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THE MISSOURI BOTANICAL GARDEN.

By Professor WILLIAM TRELEASE,
DIRECTOR OF THE GARDEN.

IN 1840, Henry Shaw, an Englishman who had settled in St. Louis in 1819, retired from business with the—for that time—large fortune of a quarter of a million dollars. Revisiting the land of his

Henry Shaw.

birth, he saw how Englishmen enjoy the fruits of labor such as that by which he had acquired this large sum of money. Traveling on the

continent, he saw what the world of pleasure offers to those who are able to pay for it. But he returned to the land of his adoption, determined to make for himself there surroundings as nearly as possible comparable with those of his own countrymen; and he so far succeeded that the modest little garden with which he surrounded his country house in the suburbs of St. Louis came in time to be the pleasantest of the resorts of the residents of that city, and the one place to which they were all sure to wish to take their visiting friends.

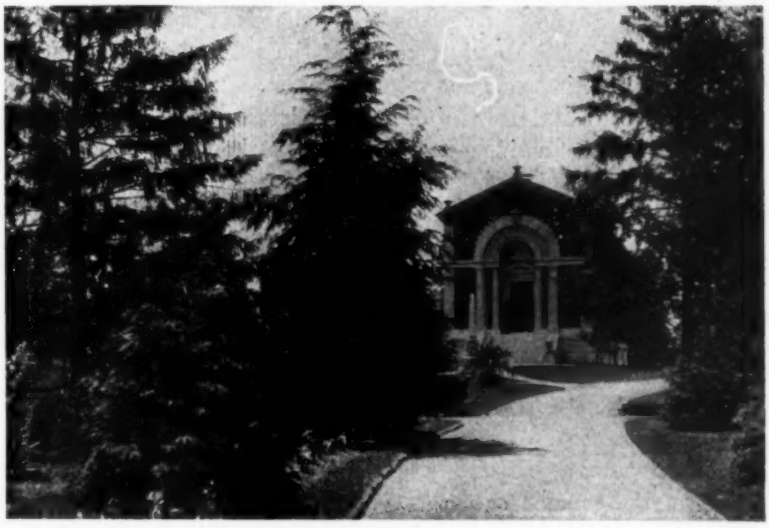
If he had stopped with this, he would have caused his name to be long remembered by his fellow-citizens, for he did not hold his home as his own exclusively, but admitted to the enjoyment of its beauties any who wished to share them with him. But while his grounds were developing and growing in beauty, he learned, through the helpful acquaintance of Sir William Hooker and of his own fellow townsman,

WALL GARDENING.

Dr. George Engelmann, that it might be possible to perpetuate his name in a surer and more lasting manner, and to cause the garden that he had planned and planted to become of use in the world of science and to grow in such usefulness through the centuries, while losing nothing of its beauty and attractiveness to those who cared to enjoy without using it. In 1859 he secured the passage of an act by the legislature of Missouri which empowered him to deed or will, as he might elect, such of his property as he wished, to trustees for the maintenance of "a botanical garden for the cultivation and propagation of plants, flowers, fruit and forest trees, and for the dissemination of the knowledge thereof among men, by having a collection thereof easily accessible; by the establishment of a museum and library in connection therewith, as also by establishment of public lectures and instruction upon botany and its allied sciences, when it shall be

deemed advisable in furtherance of the general objects of said trust; and . . . for the purpose of maintaining a perpetual fund for the support and maintenance of said garden, its care and increase, and the museum, library and instruction connected therewith."

As the new and larger plans shaped themselves in Mr. Shaw's mind, they began to take form on the grounds, and a flower garden, arboretum, and fruticetum—Loudon's main parts of a garden—were quickly laid out and planted, and separated and fronted by rubble walls in the outermost of which a severe but rather impressive gate-house, bearing the chosen name of the establishment—Missouri Botanical Garden—was built. Not far from his house, Mr. Shaw put a small fireproofed building, over the door of which the inscription 'Botanical Museum



THE MUSEUM BUILDING.

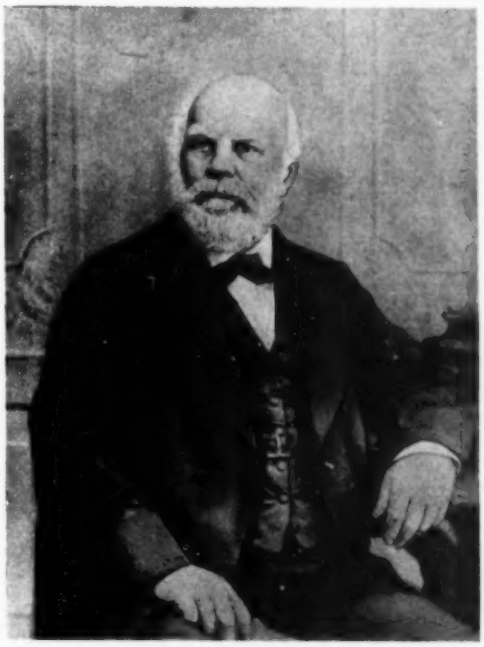
and Library' was cut in the stone, and in which, largely through the interest of Dr. Engelmann, were soon installed a small but well chosen collection of books, and some 60,000 specimens of plants, consisting of the herbarium of the then lately deceased Professor Johann Jakob Bernhardi and a small local collection made by Riehl. The arrangement of these specimens, Mr. Shaw once informed me, was entrusted to 'a young man named Fendler,' whose name was already known as that of an expert collector and destined to be made still better known by subsequent work in the tropics. On the occasion of my introducing to him a gentleman who was making a study of the flora of Missouri and who wished to consult the Riehl collection, Mr. Shaw expressed the regret with which he observed that though the flower garden was

visited by thousands of people each year, and the contents of the small and miscellaneous museum attracted them, this was the only request for access to the herbarium or library that had been made by a botanist for years.

From time to time he turned his thoughts toward the fuller realization of his plans, apparently hesitating between leaving their inception to the trustees that he had provided for appointing by will, and making the beginning himself either alone or in conjunction with trustees—a possibility specifically provided for in the enabling act of the legislature. On the occasion of my first visit to him, in the early spring of 1885, he pointed out to me the place, on Flora Avenue, before the main gate of the garden, where he had seriously thought of building lecture rooms, laboratories and residences for a faculty of botany. It was somewhat earlier than this that he called the great botanist, Asa Gray, into his counsels, and largely because of the wise advice of Dr. Gray, who saw that the time was not yet come in St. Louis for an institution such as was contemplated, he decided to let its growth be a normal one from small beginnings—but without in the least modifying his provisions for the final attainment of the largest results he had ever contemplated. Amid the beautiful surroundings of his country home, though also maintaining a city house, Mr. Shaw passed the latter half of a very long life—always coming back to the reconsideration of the coming development of his plans, modifying them in details, but never altering their original breadth as shown in the act that had been passed so many years before. To the garden he welcomed all who cared to visit it, and himself dictated the position of nearly every tree or smaller plant set out.

It was apparently the death of Engelmann, a resident of St. Louis and one of the greatest as perhaps the most accurately painstaking of American botanists, that caused the next step forward to be taken. Shortly after this, in 1884–5, Dr. Gray was once more, and this time rather urgently, called into consultation, that the city in which the plans were laid might not be entirely without a botanist; and the result was that in the spring of 1885 Mr. Shaw proposed to the directors of Washington University to endow in that institution a school of botany, with the understanding that by testamentary provision the best uses of his garden for scientific study and investigation should be ensured to its professor and students. The offer being accepted, the Henry Shaw School of Botany was formally inaugurated on the sixth of November following, and its single professor was thrown into a pleasant and frequent personal intercourse with Mr. Shaw which lasted until the death of the latter. About this time, suggestions were not lacking, from men of judgment, that by rendering the union be-

tween the garden and school of botany still closer through placing the former under the care of the directors of the university, it might be possible to ensure a much larger revenue for the needs of the garden since the university enjoys exemption from taxation under an old charter, which the new establishment could scarcely expect to secure with the existing constitutional provisions of the state. The arguments were well considered, but the saving of money—which to-day would amount to over $25,000 annually—did not weigh in Mr. Shaw's mind as contrasted with autonomy, and when he died, in August, 1889, his will was found to provide for the administration of the garden by an independent board of trustees, consisting of fifteen persons; ten named by the testator, and the other five holding office as trustees *ex officio,* in various capacities: The chancellor of Washington University, the bishop of the episcopal diocese of Missouri, the president of the public school board of St. Louis, the president of the Academy of Science of St. Louis, and the mayor of the city. Two additional honorary trustees, whom he evidently hoped to have assist in the inauguration of the establishment on broad lines, were named: Dr. Asa Gray and Professor Spencer F. Baird—neither of whom, unfortunately, survived Mr. Shaw, while it is evident from the phrasing of his will that their places were not intended to be filled. Two other citizens of St. Louis, named as trustees in the will, had died, also, before the testator, and it was held by the courts that their places should not be filled. Except for the members *ex officio,* the board of trustees is a self-perpetuating body, itself filling vacancies as they occur.



GEORGE ENGELMANN.

The testamentary provisions for the carrying out of Mr. Shaw's purposes do not differ in any essential respect from the plan sketched in the enabling act of 1859—passed because there was at that time some uncertainty as to the possibility of otherwise making the provisions

that he wished to make in such manner as to ensure their permanence. The purposes of the trust are stated by him in his will to be "having for the use of the public a botanical garden easily accessible, which should be forever kept up and maintained for the cultivation and propagation of plants, flowers, fruit and forest trees, and other productions of the vegetable kingdom; and a museum and library connected therewith, and devoted to the same and to the science of botany, horticulture, and allied objects." Provision is distinctly made for the perpetual maintenance of the establishment on the original site; for holding the endowment in the form of real estate, in which Mr. Shaw, following English custom, had made most of his investments; for the establishment of public lectures on botany and allied sciences; additions to the collections of plants, the museum and the library; exchanges; increase in the means and appliances of instruction; and

A Bit of Horticulture. Figs Prepared for Winter.

the maintenance of the revenue of the school of botany up to a specified point. It is provided that there shall always be a director of the garden, appointed and subject to removal by the board of trustees, by whom his duties are from time to time to be prescribed, but who, "when within the sphere of his duties thus prescribed and while he shall faithfully perform those duties thus prescribed . . . shall not be subject to the interference, management or control of said board," though without such construction of this provision being possible as to take away from the board the permanent control over the garden and the grounds of the institution. The director's residence is indicated as the garden home of its founder, and he is required by the will to devote his entire time and efforts to its interests, and to so employ his energies that from year to year the institution shall grow up in efficiency in promoting the ends in view in its inception.

Provision is made for the cooperation of the garden and the school of botany by the requirement that the professor or professors in the latter shall be the director of the garden or his chief assistant, or both, or that they shall be appointed on nomination of or subject to the approval of the trustees of the garden. The instruction of garden pupils is specifically indicated as a purpose of the institution, and among the subjects that are mentioned as forming a part of the purpose of its founder are horticulture, arboriculture, medicine and the arts, so far as botany enters into them, and scientific investigations in botany proper, vegetable physiology, the diseases of plants, the forms of vegetable life, and of animal life injurious to vegetation, and experimental investigations in horticulture, arboriculture, etc.; but the testator wisely adds: 'I leave details of instruction to those who may have to administer the establishment, and to shape the particular course of things to the condition of the times.'

THE GARDEN HOME.

The intention and obvious need of maintaining the establishment as an ornamental garden are evident in the many references to it as a fundamental idea, and Mr. Shaw very specifically states that he considers it 'an important feature to always keep up the ornamental and floricultural character of the garden.' Direction is given that the yearly net revenue from the endowment shall be applied 'first to the payment of the salaries of the director, assistants, professors and gardeners, and the payment of the wages of the employees and laborers, in keeping up the grounds in good style and providing for the preservation and increase of the plants and trees, and preserving the buildings and inclosures of the grounds, and secondly to the purchase of plants, flowers, and trees, additions to the library, the enlargement and improvement of the garden when necessary or advisable, and such other expenditures as from time to time may be found necessary' in furtherance of the purposes of the testator.

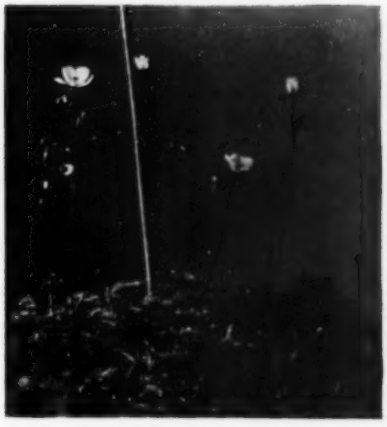
That, in the many specifications of provision for instruction and research, sight should not be lost of his wish that the institution, as a place of beauty, should always afford the pleasure to coming generations that it had given to the people of Mr. Shaw's own day, and that its connection with the outside world might be a pleasing, helpful and broad one, he provides that, though closed on holidays and Sundays as a rule, it shall be opened on the afternoon of the first Sunday each in June and September—dates on which it presents at their best two distinct phases of its attractiveness, the roses and other spring shrubbery on the one, and the decorative bedding on the other; that there shall be preached each year, by a preacher and in a church selected by the episcopal bishop of the diocese of Missouri, a sermon on the wisdom and goodness of God as shown in the growth of flowers, fruits,

SUNDAY VISITORS.

and other products of the vegetable kingdom; that premiums may each year be awarded at a flower show in St. Louis; and that each year there shall be given a banquet to the trustees of the garden and the guests they may invite—literary and scientific men, and friends and patrons of the natural sciences, and a banquet to the gardeners of the institution and invited florists, nurserymen and market gardeners of St. Louis and vicinity.

Immediately after Mr. Shaw's death, the admittance of his will to probate making known the constitution of his board of trustees, the latter organized and appointed as director the professor selected by Mr. Shaw to take charge of the school of botany, defining his duties as 'the duties prescribed for that office in the last will of Henry Shaw, deceased, and such other duties as may from time to time be pre-

scribed by this board in pursuance of the trusts declared in said will.' Under its organization, the board holds monthly meetings for the transaction of business connected with the management of the large endowment property—which at the time of Mr. Shaw's death was appraised at not far from a million and a third dollars, and which is now carried on the books as $1,588,274.60—and the consideration of current administrative details of the Garden. The board consists of some of the most representative citizens of St. Louis, and is possessed of the fullest confidence of the community, as is shown by the attitude of the courts, when, as has several times proved desirable, instructions have been asked on questionable points, or special powers requested. The most notable instance of this is afforded by a request of the board for power to sell certain endowment real estate left by Mr. Shaw and distinctly made inalienable by the terms of both the enabling act and his will, but which was found incapable of utilization for long-term residence leases, as contemplated by him, because of the unwillingness of American home builders to make use of leased ground. Notwithstanding the clear provisions against the alienation of real estate, the courts, being convinced that these provisions brought a detail of the will into conflict with its purpose, granted the desired permission after full consideration of the question in both the lower and supreme court. By the provision of small committees charged with specific duties, the board is able to give remarkably detailed care to the many phases of its trust, and from month to month the plans of the director for the administration of the establishment are passed in review and provided for by suitable appropriation of funds.



A CARNIVOROUS PLANT—*Drosera brevifolia.*

In the development of the Missouri Botanical Garden, two distinct periods are already distinguishable: a first, now drawing to an end, in which, because of the unproductiveness of a very large part of the endowment property, and the need of protecting the latter by the accumulation of a reserve fund sufficient to cover improvement costs that might at any time be assessed against it, little could be hoped for except the bare maintenance of the institution on the lines indicated by its founder, which, having been inaugurated by him only in part, at once increased the expense of maintenance considerably beyond the

point at which it had been when the garden was only the home of a private gentleman; and a second, now in sight, in which the realization of his plans to make of the garden a center of research and instruction is to be looked for in greater measure with the passage of every year.

During the first, or maintenance, period, both trustees and director

GENERAL EFFECT—THE GROVE.

have taken the ground that standing still was impossible—that the institution must either advance or recede—and maintenance, therefore, has really meant slow growth in most of the directions contemplated by its founder. In one single respect, only, has ground seemingly been lost—for the small museum, which, in the later years of

DETAIL—A MOSAIC OF SUCCULENTS.

Mr. Shaw's life, under the care of household servants had deteriorated so far as to be neither useful nor creditable, was closed when his trustees took charge of the establishment; and the need of using the museum building for other purposes has as yet prevented renovating and reopening it. Under the liberal policy of his trustees, the collec-

tion of living plants has increased from a scant 2,000 to 10,000 species or varieties; the decorative features have each year grown in variety and attractiveness; the library has increased from less than 1,000 volumes to over 36,000 books and pamphlets; the herbarium has increased from a little over 60,000 sheets of specimens to about 400,000 sheets; a course of instruction for garden pupils has been put in operation, which has trained a number of the best of the young men now engaged in horticulture—in the broad sense—in the country; the school of botany, though it has not had many students, has given botanical instruction to such of the undergraduates of Washington University as wished to take up this study, either as pure botany or in connection with medicine or engineering, and has prepared for the Doctor's degree in the university several candidates whose theses have reflected credit on the institution as well as on themselves; and, during the laying of what must be conceded as a solid foundation for the more rapid development and greater productiveness of the next period of the garden's history, time has been found by the garden staff for the performance of sufficient research work in various departments of botany and horticulture to have caused its recognition as an establishment for this purpose.

A SPECIMEN PLANT—*Martinezia caryotæfolia.*

During the laying of the foundation for the greater productiveness of the garden, sight has not been lost for a moment of the desirability of maintaining it as an attractive resort for the lovers of the beautiful, and it may be said that considerably over 100,000 persons visited it last year—some 43,000 on the two open Sundays. As is always the case in large places, detail is often lost in mass effect, or the seeker after detail sees nothing of broad treatment; and in the administration of the institution there is not a day which does not bring to the director more dissatisfaction with either general effect or detail than is

made good by contemplation of the best that either offers. And yet the impression that the garden makes on those whose comments reach the office, appears to be that of a place of beauty—and it is often a matter of surprise when I become critical of the dwarfing of individual plants that attends their massing either in the open air or in the

THE MAUSOLEUM.

crowded plant houses, to find that excellent specimen plants of hundreds of species are capable of disentanglement whenever they are needed for any particular use or can be allowed adequate space.

At the time of Mr. Shaw's death a fraction less than forty-five

IN THE PARTERRE.

acres of land were included in the garden, divided into: garden proper, 9.4 acres; arboretum, 20.5; fruticetum, 8; lawn about residence, 2.7; grove about the mausoleum of the founder, who, by his own direction, is buried on the grounds, 0.6; and vegetable garden—the garden first laid out by Mr. Shaw, at the rear of his house—3.5 acres. Though

changes cannot be held off indefinitely, the several parts of the garden as existing at the time of the organization of the board of trustees have thus far been maintained in much the form that they presented during Mr. Shaw's life.

ONE OF MR. GURNEY'S LILIES.

The central garden consists of a sunken parterre immediately facing the main gate, through which most visitors enter the grounds; a series of regular beds separated by low hedges, lying to the left and centered about a pavilion from which a bird's-eye view of the whole is had; and a group of plant houses, with lawn and bed surroundings,

WILD FLOWERS AMONG THE TREES.

at the right. It is on this part of the garden that the largest annual expenditure for maintenance is made, for there is no class of open-air gardening so expensive as flower-gardening in beds separated by lawns that are kept properly mown. Among the additions that have been

made to this part of the garden are ponds for the cultivation of the Royal Lilies (*Victoria Regia* and *V. Cruziana*) and other water plants —a group which is a particular favorite of the head gardener, Mr. James Gurney, who has originated several beautiful and remarkable hybrids and seedlings in it.

The arboretum, which for some reason was planted with the trees in rows, as in a nursery, has always been kept in a less polished condition than the flower garden, affording opportunity for the spontaneous growth of many of the wild plants of the region—indeed, a portion of it has never been plowed, and its prairie vegetation is left undisturbed by the scythe until after the year's growth is finished. It contains a varied collection of trees, many of them now of mature growth, though the tornado which devastated St. Louis in 1896 destroyed some of the choicest of them and mutilated others. In its park-like character, the arboretum affords a restful change from the more formal flower garden, and its studied air of neglect is not the least of its charms. With the growth of the manufacturing interests that have necessarily followed the lines of the great railroads passing not far from the northern and western limits of the garden, has come a considerable change in the possibility of growing perennial plants, and particularly coniferous evergreens, which, intolerant of smoke to a degree scarcely surpassed except among the lichens—which I have never seen growing in the garden—have gradually succumbed, until, notwithstanding constant efforts to replace them, they have all but dropped out of cultivation.

IN THE ARBORETUM.

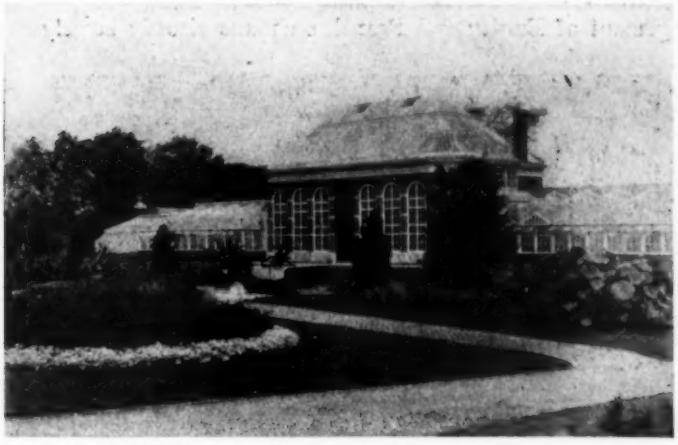
In the so-called fruticetum, which, from the nature of its present use, is kept closed to the ordinary sight-seer though always opened to those to whom its contents are of real interest, is growing, in addition to the shrubbery properly constituting a fruticetum, a small collection of the fruits best adapted to the climate of St. Louis, replacing

the old orchard into which, in the later years of Mr. Shaw's life, this section had grown, but which was almost entirely destroyed by the tornado.

The vegetable garden furnishes to the pupils being taught under the founder's will, the means of practise in vegetable growing, which,

A MUSHROOM BED—*Agaricus amygdalinus.*

by the provision of a small forcing house, is carried through the entire year. About the old residence, which, by direction of Mr. Shaw, is

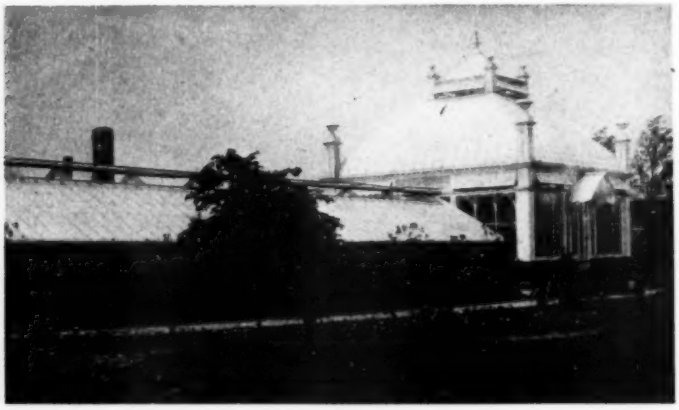


THE OLD RANGE.

occupied by the director, is still maintained the ample expanse of grass which has always characterized this part of the grounds; this is augmented by a shaded and shrub-planted strip around the vegetable garden, which Mr. Shaw called the pleasure ground, and which has been

the scene of some of the pleasantest entertainments of a bygone generation of St. Louisans.

Adjoining the arboretum and vegetable garden is a pasture area of some eighty acres, of which one fourth has recently been graded, drained,



MODERN HOUSES.

supplied with water, and partially planted to a synopsis of North American plants arranged in the familiar sequence of families adopted by Bentham and Hooker, while the remainder will shortly be molded and planted to a general synopsis exemplifying the more modern arrangement of Engler and Prantl, on plans already largely prepared.

A CACTUS HOUSE.

Gardening within doors was not a large part of the art as formerly practised, and the plant houses are far from meeting the wishes of the gardeners, even to-day. The earlier of these were built before the days of light and airy steel construction, and to grow symmetrical

and elegant specimen plants, or to bring along through the season a constant succession of flowers, was not and is not possible. In them, however, are to be seen choice or picturesque specimens of a variety of plants best adapted to the conditions that they offer, and of late years they have been supplemented by a range of modern houses, which, though small, is sufficiently large for the growth of many plants that are not seen outside of botanical establishments, as well as of those that possess commercial value because of their ready growth and abundant production of flowers. Among the collections of tender plants that are especially worthy of mention are the cacti and agaves, which, the special subjects of much of the work of Engelmann, have long been well represented in the living collections, and have been added to with the passage of time until there are today few collections of these groups which surpass or even approach them in size or importance. One house is devoted to representatives of the Bromeliaceae, of which something over 100 species are cultivated, and to which others are being added at short intervals. One tower is occupied with tender yuccas, which, planted in the ground, are beginning to assume a size and character impossible in the open air in a latitude so far north as that of St. Louis, or in tubbed specimens. In one house are brought together the sago plants, Cycadaceae, of which a fairly good collection is owned by the garden, and added to with every opportunity. In another, are planted out tree-ferns. In others, orchids, already numbering some 600 forms, aroids, in considerable variety, carnivorous plants, acacias and plants of similar foliage, and other groups of particular decorative, economic or biological interest, are displayed, and the provision this season of several new houses for the propagation of plants and for growing those that are needed for

AGAVES.

the embellishment of the houses that are open to the public, makes possible increased beauty of the latter.

Vegetable gardening is not ordinarily attractive, and a truck garden is usually associated with compost piles, bad smells, and disorder. It is not necessary for this to be true, however, and the little vegetable garden connected with Shaw's Garden is not only not unsightly in itself, but it has from year to year afforded the means of cultivating in the fullest variety a number of kinds of vegetables. Here, for instance, were grown for several seasons all procurable varieties and spontaneous species of chiles, and the monograph on *Capsicum* which the horticulturist of the establishment published as a result of the studies that he was thus enabled to make, takes rank as of value alike to botanists and gardeners. Here, too, have been grown, year after year, the chief varieties of beans that the world's markets afford, and on the plants so cultivated has been based another and equally useful monograph, by the same gentleman, Mr. Irish. Though not classed as vegetables, sweet peas in all procurable sorts have been grown for like purposes of study in this enclosure, its seclusion affording needed protection of the plants while under observation; the thesis of one of the garden pupils, Mr. Rudolph Mohr, on this group of decorative plants, is of more than passing interest.

THE NEW ORCHID.

It is the lot of all living and growing institutions which give promise of prolonged existence, to have gifts of greater or less magnitude made to them. The Missouri Botanical Garden has not proved an exception to this rule. Even before the death of its founder, Dr. George J. Engelmann placed in the hands of the present director of the garden the invaluable herbarium of his father, the late Dr. George Engelmann, and shortly after the organization of the board of trustees

this was formally transferred to them by Dr. Engelmann, together with the library of his father, which, however, had actually been placed at the garden and in part arranged before the death of Mr. Shaw. One of Dr. Engelmann's biographers expressed the great surprise occasioned by the vast amount of work that he, a busy physician, had found time to do. The number and minute accuracy of his unpublished notes, which form part of this gift from his son, were even more surprising. Over 20,000 of them exist, varying in character from a mere memorandum of the appearance of a plant which he had observed in a foreign garden, or a simple bibliographic reference, to accurate detail sketches of all of the specimens on which his conception of a species in a difficult group rested; and the sixty thick volumes in which they are now contained are counted among the choicest possessions of the institution.

E. Lewis Sturtevant.

It was but a few years after the organization of the garden under its present management that the Director one morning received a letter from the late Dr. E. Lewis Sturtevant, a correspondent for many years, but one whom he never had the privilege of meeting personally, asking if the garden would accept a large collection of specimens, reference cards, sketches, partly in water-colors, and other material illustrative of the genus *Capsicum*, with a view to its ultimate utilization in the preparation of an illustrated monograph such as the donor had for years had in contemplation for his own pen, but which he then saw that he must place in other hands. The gift was accepted, and the resulting publication, which has been referred to above, is now a matter of history and, I am pleased to say, met with Dr. Sturtevant's approval. Some years later, stricken with mortal illness, Dr. Sturtevant once more wrote to ask if the garden would accept as a gift the large and important collection of pre-Linnean books that it had been his pleasure to accumulate through the years of his active study of the origin and modifications of the plants cultivated by man, adding that he wished to attach no conditions to the gift if it would be accept-

able, though he offered the suggestion that the best use of the books would be secured if they might be separately shelved from other classes of books and if a catalogue of them and the other pre-Linnean books of the library might be printed, so that persons wishing to make use of them might know where they were to be found. Visitors to the library are more interested in the quaint volumes of this part of its contents than in any other, and investigators have called its treasures into use on many occasions, not infrequently writing from a distance for transcripts of passages that they have needed to use, but have not been able to consult elsewhere. Sometimes, for days at a time, students of particular groups of plants long cultivated, have been occupied with the camera, reducing to unimpeachable form their quotations from these books. Quite recently, it has been found possible to nearly double this collection, by the purchase of a similar collection formed by a gentleman in Europe, and the catalogue of the Sturtevant library —made and printed some years ago in compliance with the suggestion of its donor, which was considered of the greatest importance—will be supplemented by a list of these later acquired works. Very touching was the last thought of Dr. Sturtevant for the garden. Knowing that death was at hand, he took from their cabinet the last and greatest of his treasures, his manuscript card references to the literature of cultivated plants, and himself packed them in a safe manner for shipment, giving instruction that they should be forwarded to the garden immediately after his death. May the institution never be less worthy than it then was of such confidence!

IN THE LIBRARY. A Mantel of the City Home.

To go into details concerning the library at large would be wearisome and useless. Suffice it to say that, based on the small but well selected library bought by Dr. Engelmann for Mr. Shaw many years before his death, and augmented by the gift of the libraries of Engelmann, Sturtevant and the director of the garden as well as many

smaller collections, it has been increased by annual purchases of considerable size, selected with discrimination from the libraries of some of the world's greatest botanists, as these, through the death of their owners, have come into the market, until it now comprises over 16,000 volumes and 20,000 pamphlets, valued at over $60,000 and fairly symmetrical in all fields of botany and the sciences that must be taken into consideration in botanical work, as well as in gardening, landscape work, horticulture, forestry, greenhouse construction, and the like. Large as it is, however, it is so far from being satisfactorily complete for the uses it is put to that a sum greater than its present valuation could be spent on it within a few years, if the money were available and the works needed were in the market, without having even then a surfeit of the good things that such a library, destined for research purposes, should offer the busy man who, to use them, must be freed from too great care and delay in searching them out and placing his hand on their contents. Strongest in the library, as would be expected from the fact that the Garden is a garden and possesses a large herbarium and is at present more concerned with the systematic study of flowering plants than with other subjects, are the departments devoted to floras and monographs; those difficult things to have at hand, series of journals and proceedings in whole or in part devoted to botany; and the compendiums of gardening and garden plants; but there are in this country few fuller collections of treatises on plant morphology and physiology, and the works on the ecology of the flower are probably nowhere surpassed in completeness of representation. From the first establishment of the school of botany in the university, it has been the policy to spare no trouble or reasonable expense to make the library as complete as possible in any field in which special work is taken up, and the result is that each piece of research accomplished has been marked by a corresponding growth in the library.

A herbarium is an uninteresting collection to the average person who does not need to use it, whether he be a botanist or not. In envelopes or glued or bandaged down on sheets of paper are thousands of more or less fragmentary plants, sometimes moldy or worm-eaten, for time works havoc with all organic matter, and usually much faded. And yet not even the library is more indispensable for the worker on the species that they represent; for they are the real plants, and not some one's interpretation of them. The choicest part consists always of the original specimens preserved by an author when describing and naming a species or genus, for however his description or figure may have erred, this type persists as a record showing the true generic and specific characters. An herbarium is not always a conclusive source to which to turn for final information, for the author may have and

often has had two or more related species under his eye when describing the one to which he gave recognition and a name, and herbarium study calls for some discriminating power; but notwithstanding the inherent difficulties, its value is real and lasting. Though the founder of the Missouri Botanical Garden did not specifically mention a herbarium in his will, his purchase of the Bernhardi collection in the early years of his planning shows his practical appreciation of the need of such a part of the equipment of the institution, and in some manuscript suggestions for his trustees, not made a part of his will, he distinctly states that the correct naming of the plants cultivated in an educational and research garden 'can only be done by a botanist, aided by an herbarium and botanic library'; and one of the world's greatest botanists, in speaking of the relative value of living collections and the *hortus siccus* or herbarium, expresses himself as follows:* "If the collections of dried plants are compared with those of living plants, the advantages of each are more nearly balanced than is usually thought. In a herbarium you see simultaneously specimens of related species and also different localities, different ages or different conditions of the same species. You know the name of the plant, if the herbarium is well determined, and you go at once to the authors who have spoken of it. You learn its origin, which is indicated on the label. On its side, the living plant gives more means for certain anatomical observations. It permits one to better describe certain characters of little importance, such as color, odor, etc., but in the country the plants are not named, and in a botanical garden they are often badly named. . . . The geographic origin of the plants is there almost always uncertain or unknown; the individuals are often modified by cultivation and crossing; fruits are rarely seen with the flowers; rarely several individuals of the same species or several related species; and still more rarely are botanists permitted to gather enough specimens of an exotic plant to examine it to their satisfaction and to preserve the proofs of their work." And he goes so far as to head the chapter devoted to this consideration with the lines: 'Of herbaria in general and of their superiority to every other zoological or botanical collection.' It might have been added, truthfully, that in a garden the representation of any given species is likely to be transient, since the casualties to which living specimens are liable are innumerable; on the other hand, the specimens in the herbarium, though subject to their own particular dangers, are far less likely to suffer, and rarely disappear even in the course of very long periods of time except as a result of gross carelessness.

The principal collections of this class at the Missouri Botanical Garden are, in the first place, the herbarium of Engelmann, which,

* De Candolle, La Phytographie, p. 365.

containing the types of his work on cacti, *Agave, Cuscuta, Yucca,* conifers, and other groups of plants on which he was a recognized authority, and in which he described many species, will long form the most essential resource of students of these plants, for clearing up difficulties in their interpretation. Some years since, one of the young men at the garden was struck by the frequent coincidence of a certain handwriting with species of grasses known to have been collected by Haenke, and to have served for studies by Presl; and investigation showed that the writing was really Presl's, and that in the Bernhardi herbarium, which contains many valuable—but unfortunately inadequately labeled—specimens, was a nearly full set of this important collection of Haenke—the grasses of which were subsequently studied in detail by Professor Lamson-Scribner, to the elucidation of a number of questions concerning the grasses of the United States, which could be answered only by recourse to the original specimens on which Presl's species rested.

It would be even more tiresome to give a minute analysis of the contents of the herbarium than of the library. Suffice it to say that in addition to the Engelmann and Bernhardi herbaria, it contains the herbarium of Sturtevant—purchased after his death from an old friend to whom he had given it, but who desired it to be placed with the other material serving as a record of his friend's work; a pre-Linnean collection formed by the German botanists, Ludwig and Boehmer, on which a Prussian flora was based long since; the cryptogamic herbarium of the director of the garden; a very large and full collection of the plants of Alaska, made on the Harriman Alaska Expedition, and containing, through the Bernhardi herbarium, representatives of a considerable number of the species collected by the early Russian explorers; a good representation of the plants of the West Indies, collected by a former assistant at the institution, who was sent into the Caribbean region on a collecting trip; one of the fullest representations of plants from the Azores, in which, and to a smaller extent in Madeira, the director has spent some time; a very full representation of the flora of Missouri, in which the Riehl collection and a set of specimens many years since put up by Professor G. C. Broadhead, and on which his early notes on the plants of the state rest, are of especial interest; and probably the fullest set of Chinese plants that has yet come to this country. All the more important current collections of the plants of North America, and many of those from foreign lands, have been bought as they have been offered for sale. Not long before his death, Dr. Chapman, long the only authority on the flora of our southern states, asked the director to visit him, in order that arrangements might be made for the ultimate

passage to the garden of his personal herbarium. Twice, the Chapman herbarium had been sold—each time the best representatives in it being selected, and supplemented by fragments from his scrap books when these were essential for the representation of a species; but there remained at the time of his death a large amount of material which as he said consisted of the original fragmentary specimens representative of his earlier work on the southern flora, and, therefore, really the types of the species contained in it as he then understood them. For every student of the plants of this region, therefore, this collection is of the greatest value, as showing, so far as

SEED BEDS.

it goes, what he really had in mind when using a particular name—and it was precisely that this record of his work might be permanently preserved, that he was desirous that his collection should find its home in the garden.

In themselves, living plants, books and herbarium specimens are but a burden to those charged with looking after and caring for them. He is a happy botanist who has no care except for the thing that his mind is turned to at any given moment. Happy the gardener with only a bay-window to care for! The need for accumulating more than the use of the moment demands lies in the impossibility of otherwise having at hand facilities when they are needed. The sight-seer finds nothing but a curious scene in half an acre of seed beds, thickly studded with little labels which mark the lines where thousands of kinds of seeds lie awaiting the quickening action of sun and rain; but to the student of morphology its value is untold. The sightseer, too, is impressed with but a small part of the things that he sees in passing through a large collection of even the most beautiful plants. A mass of color, a clump of graceful foliage, repeated in different

forms and places by the use of perhaps a scant score of species, is all that the average mind carries away from the greatest botanical establishment. The difference between five hundred and five thousand species in a collection is utterly lost on the casual observer. Let him, however, wish to see some particular plant that he has chanced to read of, and the difference becomes evident. To the botanist, even, the difference between one thousand and ten thousand species is not readily perceptible until he has need of some particular thing, which the larger collection may afford while the smaller is almost certain to offer only disappointment.

However large or small it may chance to be, any collection is useful or valueless according as it does or does not give information as well as please the eye. Curiosity, alone, prompts nearly every

A NAMED SPECIMEN.

observer to look for the name and native home of a plant that he sees growing in a garden. Much more than this is desired by some, and could be added profitably for all were it not for the fact that more deters the ordinary visitor and so defeats the very object for which it is offered, by keeping him from reading at all. No small part of the usefulness of a botanical garden lies in giving information that is not sought, and that the recipient would not himself think of seeking, but which reaches him through such natural channels that he unconsciously acquires not only it, but the habit of looking for the same kind of information about other things, and for more on the particular one that he has first become interested in. One of the principal objects of the founder of the garden therefore is served by the simple presentation of a named collection of attractive and attractively placed plants, which educates while pleasing those who see it. This is varied by the provision of special collections that are tempting

in themselves—such, for instance, as a part of the grounds devoted to plants that are hardy in the city. Thousands of persons each year see in this little collection something that by its beauty or oddity attracts them, and the provision of a general label showing that all are hardy, and of the individual label, telling its name, causes them to wish it for their own garden and enables them to order it of their florist, or, if he does not know how to supply it, puts them in the way of seeking it in the lists of other dealers.

Very frequently, parties of visitors are accompanied through the garden by an employee who points out to them the more important features or aids them in finding things in which they are particularly interested. One portion of the grounds contains, in sequence, the principal families of higher plants, represented by several hundred attractive species chosen with reference to the fullest possible generic representation. Another is set aside for the growth of medicinal plants, and elsewhere are grown representatives of the commoner agricultural crops, of savory herbs, etc. These collections, in particular, are much used by teachers in the public schools, who bring classes to the garden for a part of their nature work, in the open air.

Even surplus and waste material contributes in a way to the same end. It is always necessary to propagate bedding plants in excess of the number actually needed, so that replacements may be possible in case of accidents or inclement weather following their transplanting into the open. While only two or three permanent specimens of a kind are wanted for tender plants, most species grown from seed are sure to be germinated in a greater number of individuals than this. When frost necessitates the clearing of the beds in autumn, many plants that have served their purpose during the growing season are still living, though disfigured. This surplus or refuse material is not thrown away, but by the expenditure on it of a very little care is potted, set aside in frames for recuperation if this is necessary, and distributed to hospitals and schools, where, particularly in the poorer and more densely populated parts of the city, it quietly continues the refining influence that Henry Shaw appreciated as one of the most desirable effects of his garden in his own time.

But the great ultimate purpose of a botanical garden is the fostering of research with the purpose of adding to knowledge. This was recognized by Mr. Shaw, and repeatedly mentioned in his provisions for the future of his establishment. Opportunity for it lies in every plant in the collections, and in every book on the shelves. To get men to use the opportunities offered is the greatest problem of administration. So far, the garden has employed only the most necessary care-takers, and it would have been impossible for a single one of its employees

to have been spared from the force without neglect of some essential. Yet, though over-work and a permissible if entirely undesirable neglect of details of convenience in use, but not of preservative care, have been necessary, each of the higher employees has every year found time to do something worth doing in the field of investigation. The achievements, it is true, are neither as many nor as great as the workers or the management of the establishment would have wished them to be, but considering the fact that the garden has been in course of transition from the pleasure grounds of a gentleman to a scientific establishment, and that what has been done has been carefully done, they are not insignificant or unworthy, and each of the thirteen annual reports on the institution thus far printed contains at least one scientific publication of original results, of permanent interest to botanical workers. What will come of the staff of capable investigators that it may shortly be expected to gather together, is a matter of conjecture only—but the conjecture refers rather to the success with which men may be selected than to the opportunities that they will enjoy for the most earnest and serious application of which they are capable.

Research is coming to be recognized as of greater value for the practical development of our natural resources, with the passage of every year. The investigator sometimes sees in his subject only a problem that he must solve whether its solution can ever be of value or not. Sometimes he appears to be so constituted that the suspicion that it can result in anything useful is deterrent to him. Sometimes his chief interest lies in the very possibility of its utilization. But, in any case, no fact well made out and properly correlated is valueless, and the results of the most unpractical of discoveries are often utilized in commerce or in the arts with surprising promptness. While the research thus far carried on at the garden has been dictated largely by consideration of the needs of botanical science alone, or the personal interest of the investigator, I should not like to close this reference to it without mentioning that the studies of one of the intructors in the school of botany, Dr. von Schrenk, have taken the direction of the causes and means of prevention of the decay of timber, on which, under the authority of the National Department of Agriculture, he has done work which has brought him merited scientific recognition, while at the same time its practical results in the saving to railroads and other large users of timber already promise very large financial returns to the community at large.

While its own staff is, therefore, reasonably certain to utilize the facilities of the garden in an ever growing degree for the performance of research work, the results of which are and will always be an ample

justification of their acquisition, it is not at all improbable that the passage of time will see their utilization to an even greater extent by investigators not connected with the administration of the establishment. Whatever it possesses is freely placed at the disposal of any one capable of making good use of books or material, and while its own staff, however developed, will always comprise but a small part of the persons capable of making good scientific use of its treasures, it is probable that a recognition will grow with the years, through at least the entire central part of our country, that a visit to the garden is not only possible and practicable, but almost a necessity for all of the botanists outside of the few large universities, who desire to do in its best form the work that lies in the line of their interest and training. Much use has already been made of the garden by men who would not have done their work so well without this help, and much more use of it would doubtless be made were it not for the fact that travel and residence in a city are found more expensive than staying at home, when the isolated worker counts up the cost and consults his limited income. It may be remarked, however, that the man who counts cost too closely through life is often the man who sacrifices future usefulness to present economy, and it may safely be said that few lines of saving are less profitable in the long run than those pursued at the expense of the equipment and wide acquaintance on which professional and financial success usually rests.

Prophecies are always of uncertain value and attended with danger if the prophet seeks a reputation in that field: I shall hardly venture, therefore, to say much about the detailed future of the garden. This much, however, may be said with certainty, that its endowment appears to be so ample and so well founded that, though growth and attainment will be gradual and large immediate undertakings are not within its power, its perpetuity as an important and growing center of study and education are beyond question. The research work already in progress in the garden under the control of the government is suggestive of the continued attainment of better and larger results through cooperation with other establishments than would be possible to either alone. That the grounds will be greatly enlarged, that a supplementary garden of much larger size and more picturesque conformation will some day be opened at a distance from the smoke of the city, which has already sounded the death knell of some of the plants that were formerly cultivated with success in the present garden—which, under the provisions of Mr. Shaw's will is certain to be maintained as the real center and home of the establishment whatever adjuncts it may have—and that there will be erected in the near future a system of large conservatories of modern construction, worthy

of a great botanical garden, large additions to the single small plant house now devoted to experimental work in mycology and plant pathology, and a suitable home for its herbarium and library, with ample laboratory facilities, it is easy and safe to predict. That these shall all and always be freely at the disposal of any who wish to make serious use of them in research work of any kind, is an established policy, not likely to change. Finally, it may be said that, as its founder's wish was to make its scope broad, so the purpose of those to whom he has left its administration is to develop it, so far as the means at their disposal admit, on lines which shall better fit it with the passage of each year for the performance of useful work in any part of the field, while strengthening it to the fullest during the progress of each particular study that is taken up—and, throughout, never to let it become anything but a place to which the lover of the beautiful may turn in the full assurance that he will never find it less beautiful than when it was Henry Shaw's home, but rather a place to which wealth of scientific resource has but added greater possibilities for pleasure, when pleasure only is sought.

THE SAVING OF VANISHING DATA.

By ALFRED C. HADDON, F.R.S.,
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FIVE years ago I pointed out* that it is well from time to time to take stock of our knowledge and our methods of inquiry, to see whether we are working on sound lines. As the business man finds it necessary to go over his stock periodically and to balance his books, so, also, the scientific man, especially the biologist, should perform an analogous operation, lest perchance he finds out too late that he has been entering on a comparatively non-profitable work, or has been neglecting valuable opportunities. While it is impossible to say that any scientific work will be ultimately unprofitable, it is right to point out that particular subjects for investigation may be of more immediate importance than others.

In order not to complicate the question, we will dismiss the practical applications of science by admitting that they are of immediate importance. This leaves the field clear for the consideration of scientific subjects which are studied solely for their own sake.

We can, perhaps, gain a clear view of the question by looking at it from the standpoint of our successors. What will be the opinion of the naturalist of a hundred, or of a thousand years hence of the work now being done? What is the scientific work he would wish us to have undertaken? This question is not a difficult one to answer.

He would not consider it very necessary for us to elucidate the structure, development or physiology of every common animal; these researches can be pursued at any time. The investigation of the life in the oceans—whether on the surface, in shallow water, or in abysmal depths—can be done by him as well as by us.

The naturalist of the future will certainly and most justly complain if we busy ourselves entirely with problems that can wait, which he can solve as well as we, and at the same time neglect that work which we alone can do. Our first and immediate duty is to save for science those data that are vanishing; this should be the watchword of the present day. Those students of botany, zoology and anthropology who have at all considered the matter are impressed with the fact that the present time is a very critical period for the native flora and fauna of many parts of the world. Owing to the spread of commerce, the effects of

* *Nature*, January 28, 1897, p. 305.

colonization, and the intentional or accidental importation of plants and animals, a very rapid change is affecting the character of the indigenous life of numerous districts. This is notably the case in oceanic islands, the area of which is often extremely limited, and whose native forms have been found to be specially liable to be swamped by the immigrants; but it is just those spots which are of especial interest to the naturalist, on account of their isolation from the great land areas. Thus the flora and fauna of many of the most interesting districts for the field-naturalist are in our day becoming largely exterminated before they have been adequately recorded. The investigation of disappearing animals and plants can, in many cases, be undertaken by us alone—and even now much has disappeared and more is fast passing away. Attention has been called to the spread of land species by the agency of man by Mr. L. O. Howard, of Washington. In this very interesting and suggestive article* he deals more especially with insects and most of his illustrations are drawn from America.

In other parts of the world the same dislocation of the indigenous fauna is taking place and even the flora is also becoming modified, for example, Sir Walter Buller, F.R.S., has stated† that all the more interesting birds of New Zealand are passing away. Not a few species have already been exterminated, many more are on the borderland, so to speak, of final extinction; and some even of the commonest birds of thirty years ago have become so scarce that it is difficult to know where to look for them. The saddest part of it is that it seems hopeless now to arrest the evil, owing to the introduction of stoats, weasels and ferrets that are now swarming over every part of the country and defy all attempts to check their increase. The following facts speaks for themselves. No specimen of the once very abundant New Zealand quail (*Coturnix novæ zealandiæ*) has been seen for a quarter of a century; of the celebrated *Notornis mantelli* only three perfect specimens have been obtained; it is probably extinct. Even the extremely abundant woodhens (*Ocydromus* sp.) are on the verge of extinction, as are the various species of kiwi (*Apteryx*), the great ground parrot (*Stingops habroptilus*), the stitch-bird (*Pogonornis cincta*), the bell-bird, the native robin, and many others, not forgetting the beautiful huia or mountain starling (*Heteralocha acutirostris*), celebrated in Maori song and tradition. The huia, which is greatly prized by the Maoris on account of its tail feathers—for personal adornment and as a badge of tribal mourning—has, from time immemorial, been confined to a narrow strip of mostly mountainous wooded country forming part of the old Wellington Province.

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* *Science*, N. S., Vol. VI., September 10, 1897, p. 382.

† 'The Vanishing Forms of Bird-life in New Zealand,' *The Press*, Christchurch, N. Z., January 11, 1897.

It is only just to the New Zealand Government to point out that several years ago the provisions of the 'Wild Birds Protection Acts' were extended so as to include some species that were being destroyed indiscriminately and Little Barrier Island and Resolution Island have been set apart as bird preserves.

Sir Walter goes on to point out that there is just a chance that, in the course of time, some of these vanishing species may learn to adapt themselves to the new conditions of things, and take a fresh lease of life. The tooth-billed pigeon (*Didunculus strigirostris*), a native of the Samoan or Navigator Islands, was supposed to be rapidly becoming extinct, as its terrestrial habits rendered it an easy prey to predatory animals, such as cats and rats, introduced into the islands from European vessels; but late accounts show that it has changed its habits, feeding or resting exclusively on large trees, and that it is now increasing in numbers. Commenting on this, Professor Newton says:

> It is in this way, through the struggle for existence, that habits which have been transmitted from parent to offspring through unknown generations are suddenly abandoned, and entirely opposite ones adopted that give the needed protection to life and continued prosperity, which the inherited methods no longer are able to secure.

Now, singularly enough, the whitehead (*Clitonyx albicapilla*) was forty years ago the commonest bird in the North Island, and at that time a strict inhabitant of low scrubby vegetation, where its habits were gregarious. For many years it seemed to have become extinct, Mr. Reischek, during several years' hunting in the Auckland woods never having met with a single example. During the last few years it has reappeared, but in an entirely new character, as the frequenter of the highest tree-tops, and it appears to be sensibly increasing. On the Little Barrier, however, where it has never been much disturbed, it still continues to frequent the low vegetation.

If so marked a change is apparent on a large land surface like New Zealand, how much more rapid and effectual must be the change in small islands. There is an interesting example of this in the Hawaiian group. In 1890 a committee was appointed by the British Association to investigate the zoology of those islands. The committee secured the services of Mr. R. C. L. Perkins, who has proved himself to be a most efficient collector; his investigations prove that quite a noticeable decrease in the indigenous fauna is taking place each season. The district around Honolulu was perhaps originally the richest in endemic forms, but now introduced forms are in vast preponderance; the distinctive fauna of the plains, if there was one, has quite disappeared. Captain Cook found certain birds, for example, near the shore; of these, some are extinct, and others are to be found only in the mountains.

In a letter to S. D. Sharp, dated Lihue, Kauai, July 21, 1896, Mr. Perkins states:

> This place has been a dead failure. The country where I camped here was a low-lying, densely-covered forest bog-land, at first sight a paradise for Carabidæ (ground beetles), and differing from any other place known to me. Its fauna is entirely lost forever. I turned during my stay thousands of lop, any one of which at 4,000 feet would have yielded Carabidæ. Of all these there was not a single one under which *Pheidole megacephala* had not a nest, and I never beat a tree without this ant coming down in scores.

This is an introduced ant which is overrunning the islands, and which exterminates the native insect fauna. Mr. Perkins finds that earwigs alone can withstand this ant, and his only chance of collection of endemic insects is to get ahead of the ant. In the 'Report' for 1900 it is stated that on his return from a visit to England Mr. Perkins found that great changes had taken place in the islands during his absence, and that the forests were being extensively destroyed and replaced by sugar-cane. The grants by the British Association have been supplemented by grants from the Government Grant Committee administered by the Royal Society, and from the trustees of the Bernice P. Bishop Museum in Honolulu. Eight parts of the three volumes intended to form the 'Fauna Hawaiiensis' have now been published and others are in the press. The inception of this investigation was due to Professor Alfred Newton, and if he had not persisted until he succeeded, comparatively little would ever have been known about the fauna of the Hawaiian islands.

In a communication to *Nature** Mr. Perkins says that few countries have been more plagued by the importation of insect pests than the Hawaiian Islands; in none have such extraordinary results followed the introduction of beneficial species to destroy them, of the effect of which he gives many instances. He goes on to say:

> Why has the success of the imported beneficial insects been so pronounced here, while in other countries it has been attained in a comparatively small measure? The reason, I think, is sufficiently obvious. The same causes which have led to the rapid spread and excessive multiplication of injurious introductions, have operated equally on the beneficial ones that prey upon them. The remote position of the islands and the consequently limited fauna, giving free scope for increase to new arrivals, the general absence of creatures injurious to the introduced beneficial species, and the equality of the climate, allowing of almost continuous breeding, may well afford results which could hardly be attained elsewhere on the globe. The keen struggle for existence of continental lands is comparatively non-existent, and, so far as it exists, is rather brought about by the introduced fauna than by the native one.

To this Mr. Howard adds:

* *Nature*, March 25, 1897, p. 499.

One prime reason of success is that the most successful of the imported species have come from another portion of the same great faunal region, while others have been received from the region most nearly allied, viz., the Oriental.*

Mr. Perkins then turns to the darker side of the picture for the naturalist's point of view and forecasts what will be the result of all these importations on the endemic fauna. He says sooner or later the greater part of this most interesting native fauna is in all probability doomed to extinction.

Investigations such as those here advocated should be undertaken by a competent naturalist. He should not only be a good collector, but a keen observer, in fact, a naturalist in the true sense of the term; for unless the work is well done it had almost be better left undone. There are many examples of collecting being so imperfectly done as to lead to very erroneous conclusions. It takes time for a naturalist to become acquainted with the local types. The endemics do not show themselves, as usually the conditions of life are such that insects, for example, live retired lives and are not seen, while those that manifest themselves are often foreigners.

It is, perhaps, scarcely necessary to point out that the biological investigation of islands is not a matter of interest to the systematist only, but it is of great importance in connection with the problems of geographical distribution of animals and plants, some of which open up fascinating vistas of the extension of continents in former ages and of their partial submergence, while others relate to the when and how of the peopling of remote islands. Then there are to be considered the bearing of specific and individual varieties on the intricate questions of the origin of species and of evolution in general, and the adaptation of peculiar forms to their particular localities as well as those wonderful inter-relations between plants and plants, plants and animals, animals and animals, and between all and their environment. In a word, all those problems which are to be classed under the term ethology† require painstaking and immediate study; probably no branch of the study of life is of such pressing importance as this, for everywhere 'the old order changeth' and it is that 'old order' which we have to discover.

The extermination of animal life is more rapid and striking than that of plants, but what has been stated for animals applies equally to plants.

More than twenty years ago the late Professor H. N. Moseley raised a note of warning and the concluding sentences of his 'Notes by a Naturalist on H. M. S. *Challenger*' are as follows:

With regard to any future scientific expeditions, it would, however, be well to bear in mind that the deep sea, its physical features and its fauna, will

* *Science, loc. cit.*, p. 396.

† 'Natural History, Œcology or Ethology,' W. M. Wheeler, *Science*, N. S., Vol. XV., June 20, 1902, p. 971.

remain for an indefinite period in the condition in which they now exist and as they have existed for ages past, with little or no change, to be investigated at leisure at any future time. On the surface of the earth, however, animals and plants and races of men are perishing rapidly day by day, and will soon be, like the Dodo, things of the past. The history of these things once gone can never be recovered, but must remain forever a gap in the knowledge of mankind.

The loss will be most deeply felt in the province of anthropology, a science which is of higher importance to us than any other, as treating of the developmental history of our own species. The languages of Polynesia are being rapidly destroyed or mutilated, and the opportunity of obtaining accurate information concerning these and the native habits of culture will soon have passed away.

Many other naturalists besides Moseley have been impressed with the need there is for the study of native races, indeed most of the zoologists who have traveled afield have turned their attention to anthropology, in the broadest sense of the term and not a few have partially, or even entirely, relinquished the study of zoology for that of anthropology. The reason is to be sought not only in the interest of the study of man, but in the conviction that the opportunities for that study are fast slipping away and fastest in those countries where the most important results are likely to be obtained.

In many islands the natives are rapidly dying out, and in more they have become so modified by contact with the white man and by crossings due to deportation by Europeans, that immediate steps are necessary to record the anthropological data that remain. Only those who have a personal acquaintance with Oceania, or those who have carefully followed the recent literature of the subject, can have an idea of the pressing need there is for prompt action. No one can deny that it is our bounden duty to record the physical characteristics, the handicrafts, the psychology, ceremonial observances and religious beliefs of vanishing peoples; this also is a work which in many cases can alone be accomplished by the present generation.

How often have I, when questioning the younger men about their old custom, been told, 'Me young man, me no savvy, old man he savvy,' and so it was. Even a quarter of a century of contact with the white man will cause the attenuation or disappearance of old customs, the memory of which is retained only by a few old men; when these die the full knowledge of the old cults dies too.

I often wonder what the ethnologist of the future will think of us. The Tasmanians have entirely disappeared and we know extremely little about this primitive people. Howitt, Tison, Spencer, Gillen and Walter Roth have done memorable work among the Australians, but if these few men had not labored how much should we really know about the meaning of the social and religious observances of a rapidly vanishing people?

Apart from the work of Codrington and the special investigations of Parkinson, Danks, Fison, Von Pfeil and a few others, how little is known about the practises and beliefs of the varied natives of the Melanesian archipelago. Our knowledge of their physical characteristics is slight; we have collections of many of the objects they make, but of what they do and think our knowledge is as insufficient as it can well be.

The case is not so bad for Polynesia, but even then most of our information is scrappy and many branches of inquiry are practically untouched. Books of travel and missionary records afford ample testimony to the great change that has come over these people. Much is irrevocably lost; but if steps are taken without delay some facts of importance may yet be rescued.

What occurs with almost dramatic rapidity or thoroughness in islands takes place also on the lowland areas where the white man comes into close contact with native races. There are many tracts of Africa which are in need of immediate investigation by trained observers.

Fortunately there is no need to point the moral for North America; although much yet remains to be done, the American anthropologists have not neglected the indigenes whom their civilization is repressing. They, too, recognize that in most cases it is only the fragments of the past that they are able to recover. What they have accomplished has been due mainly to the wise liberality of public-spirited business men.

There is no need to continue, examples could be multiplied indefinitely; our scientific literature is full of laments of the insufficiency of our knowledge of almost every custom or belief in every part of the world. Untrained observers have imperfectly recorded events of which they generally knew little and cared less. Those who have traveled are in universal agreement as to the rapid change that everywhere is taking place, and yet many anthropologists are content to measure skulls or to describe specimens in museums!

A word of warning is not unnecessary. There is still a great danger that travelers will make it their first endeavor to amass extensive collections quite regardless of the fact that a sketch or a photograph of an object about which full particulars have been collected is of much greater scientific value than the possession of the object without the information. The rapid sweeping up of specimens from a locality does great harm to ethnology. As a rule only the makers of an object can give full details respecting it, and no traveler who is here to-day and gone to-morrow can get all the requisite information. This takes time and patience. The rapid collector may get some sort of a story with his specimen, but he has no time to check the information by appeal to other natives, no time to go over the details in order to see that he has

secured them all and in a right order. It is probable that many native objects have a deeper significance than would be suspected. This can only be coaxed out of the native by patient sympathy. Some information may be 'rushed,' but the finer flowers of the imagination, the spiritual concepts and sacred aspirations, can only be revealed to those with whom the native is in true sympathy and, quite apart from idiosyncrasy, the time element is most important. No, the rapid collector does positive harm, as, like the unskilled excavator, he destroys the collateral evidence. He may add a unit to a collection, but its instructive value is reduced to a minimum; it is the gravestone of a lost opportunity.

My plea then is for investigators, not for mere collectors, as many of the former as possible and as few of the latter. There is not much difficulty in finding men willing and competent to undertake such investigations if funds were forthcoming. One point is worth mentioning: in most branches of scientific enquiry, later investigations, owing to more minute study, improved methods or a new point of view, are apt to eclipse the earlier investigations. Now this is not the case with ethnological research in the field. The earlier the observations are, provided they are full and accurate, the more liable they are to be of greater importance than later ones. Students continually refer to the oldest books of travel, and they will always do so. From this point of view it is evident that properly qualified investigators should set to work without delay. Every year's delay means that the work will be so much the less perfect. All who are concerned in any field work can have the satisfaction of feeling that students of mankind in future ages will have to consult their publications, and they have the tremendous responsibility that what they write will have to be accepted as correct as there will be no means in the future of checking it.

In order that satisfactory work may be done it ought to be continuous, and two or three good men should be always in the field; to accomplish this an income of at least $5,000 per annum is required. To insure an efficient, economical and fair administration it would be desirable to appoint a small international council of some half dozen members; the council should not be too large, as the business must be carried on by correspondence. This council would decide as to the field work to be undertaken at any particular time and as to the disposal and working out of the material that was collected. Only systematists are aware of the necessity there is that the types of new species should be deposited in the most central institution and regard ought to be paid to the special circumstances of each particular group, independent of country or nationality. Patriotism should not override the practical requirements of science.

THE AMERICAN'S DISTRUST OF THE IMMIGRANT.

By Dr. A. J. McLAUGHLIN,
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OPPOSITION to immigrants is not new. Even in the convention that framed the Constitution a minority looked with distrust at the alien. A little later came the Alien Act of John Adams's administration. Again, in 1812, the Hartford Convention proclaimed 'The stock population of these states is amply sufficient to render this nation in due time sufficiently great and powerful.' During the discussion of the bill to establish the territorial governments of Kansas and Nebraska, Senator John M. Clayton, of Delaware, introduced in the Senate an amendment confining the elective franchise to native Americans; but, although some prominent statesmen warmly favored the measure, sense of justice and a prudent regard for the national welfare triumphed over narrow race prejudice, and the amendment was lost. In the debate on the bill dealing with preemption rights of settlers on public lands, approved May 29, 1830, Senator Merrick, of Maryland, offered an amendment barring aliens from such rights; but again there were enough clear heads and broad minds to prevent the measure from becoming a law. Finally, in the early fifties, opposition to the alien culminated in the Know-Nothing movement, when misguided fanatics, actuated by insane jealousy of foreigners, not only discriminated against all aliens, but attempted actual persecution.

By 1855, however, the immigrant had proved his usefulness and opposition lessened. He had convinced the intelligent American that he was not a menace, but an indispensable upbuilding force. From this time, therefore, up to a few years ago, he was subject to little or no restriction on entering our ports. It is fortunate for our growth that the immigrant of those early years was of a caliber vastly superior to that of the immigrant of to-day. To-day the immigrants are mostly of other stock than were those who gave us their brawn and muscle and indomitable courage to conquer a wilderness. It was national economy to avail ourselves of their services. They cut down the forests, dug the canals, and built the railroads, thus making our national life possible long before it could have existed without their assistance. These were the days when the immigrants were the German, the Irishman and the Scandinavian.

Of late there has been a rebirth of distrust of the immigrant. That this feeling exists and is even stronger than ever is attested by

the numerous magazine and newspaper articles on immigration. Time and again we read protests against the 'horde of illiterates,' or the 'scum of Europe,' or the 'pauper invasion' which is 'swarming into our country.' The articles are usually the feverish output of some enthusiastic patriot who has not come in close contact with the immigrant for any extended length of time, and whose remarks are misleading, though eloquent and readable.

That the writers are as inaccurate as they are intemperate may be seen from a consideration of one of the most frequent errors into which they fall, confusion of race with nativity. For example, observe how they use the term 'Russian.' We receive a great many immigrants, good and bad, from Russia, but very few Russians. So-called Russian immigration is composed (exclusive of real Russians, who form so small a part that they can scarcely be considered a factor) of five distinct races: Hebrews, Poles, Germans, Lithuanians and Finns. The same is true in almost as great a degree of 'Huns.' The immigrants from Austria-Hungary are commonly called Huns; but, while the race line can not be so unerringly drawn as in the case of Russia, the term does not apply racially to the majority of the immigrants. The bulk of the immigration from Austria-Hungary is made up of Hebrews, Slovaks, Poles, Croatians, Magyars (the real Huns) and Germans. This confusion of race and nativity is due to the fact that the statisticians of the past took no cognizance of race, but recorded simply the nativity of the immigrant. Writers in using these statistics jumped at the conclusion that all immigrants born in Russia were Russians, and all born in Hungary were Huns. For the past few years, however, immigrants passing through the Barge Office, or Ellis Island, have been classified according to race as well as nativity. The statistics thus compiled have a scientific value.

If we examine the cause of an American citizen's distrust of the immigrant we find that it varies according to the citizen's point of view. The mechanic fears cheap competition, resulting in low wages; the stirpiculturist, noting the poor physique and low mental caliber of some of the immigrants, fears race degeneration; the reformer, or political purist, increase of crime or pauperism and the influence of a mass of ignorant voters controlled by unscrupulous political bosses; and the law-abiding citizen fears from the immigrant, not only the germ of bodily disease, but the germ of anarchy and also favorable media for its growth.

The great majority of male immigrants are not mechanics, but unskilled laborers. If they possess the qualifications that the early immigrants established as the prime requisites of a desirable immigrant —rugged physique and willingness to work—and if there is a demand

for unskilled labor and this demand is not met by Americans, then their admittance or non-admittance resolves itself, to-day as in the past, into a question of national economy. The native American does not engage in the digging of excavations, carrying the hod, or mining. No native American resents that the immigrant has turned him out of the great Pennsylvania mines. The American mechanic's objection is, however, well founded. There is quite a large class of immigrants composed of men of poor physique, with their families, admitted every year, because they are skilled in tailoring, shoemaking, baking or other trades which do not require much physical strength. These people are undesirable immigrants. They enter into direct competition with the American tradesman or mechanic, accepting lower wages and working more hours.

The American mechanic, artisan or tradesman can have no grievance against the poor unskilled laborer, who does not compete with Americans, but with other foreigners who have preceded him and who is able and willing to do work that the American will not do—work that is necessary for our industrial progress. He has, however, cause to complain of the admittance through our ports every year of the thousands of skilled laborers, tailors, shoemakers and all kinds of factory hands who fill the sweat shops and factories and tend to greatly lower the American scale of wages.

The following table gives the relative value of the different races as shown by the percentages of skilled and unskilled laborers. It also shows the percentage of each race coming here between the ages of fifteen and forty-five. It is between the ages of fifteen and forty-five that the economic value of the immigrant to the country is greatest.

Unskilled laborers are absolutely essential for our industrial progress. The demand for the necessary unskilled laborers is not satisfied by native Americans.

Skilled laborers, coming as immigrants, may or may not be advantageous to the country, but they are not necessary for our advancement. The demand for skilled laborers can, with very rare exceptions, be supplied by native American applicants. Therefore, the races having the highest percentages of unskilled laborers and of immigrants between the ages of fifteen and forty-five are the most necessary and the most desirable.

Race.	Percentage Unskilled.	Percentage Between 15 and 45 Years of Age.
Lithuanian	86.5	90.5
Slav	86.9	88.2
Magyar	83.9	87.2
Finn	82.5	85.5
Italian	75.5	79.4
Syrian	48.1	75.7
Hebrew	13.6	68.5

The reformer's reason for distrust is a grave one. It is a fact that thousands of immigrants receive qualification as voters when under the law they are clearly not entitled to it. There is no doubt that such a mass of ignorant voters constitutes a great power for evil. But the blame can hardly be charged to the immigrant; rather is it due to the unscrupulous ward politicians who thus increase their following and to the judge who grants citizenship papers without proper investigation of the applicant.

So far as the fear of loathsome and contagious disease is concerned the danger is comparatively slight. The immigrants are subjected to a rigid physical examination at Ellis Island by the officers of the United States Public Health and Marine-Hospital Service. The double system of inspection practiced there makes it practically impossible for any immigrant suffering from a loathsome or dangerous contagious disease to pass without detection. The ratio in each race of the number so affected, to the total number of that race landed, during the fiscal year ending June 30, 1901, is here given:

Slav (Pole, Slovak, Croatian)	1	in every	7,000
Magyar	1	"	6,500
Italian	1	"	3,450
Lithuanian	1	"	1,250
Hebrew	1	"	1,100
Finn	1	"	1,000
Syrian	1	"	135

As far as the fear of increase of crime and pauperism is concerned, it is true that prison statistics usually show a majority of those convicted of crime to be foreign born. But the industrious statistician loses sight of the fact that the 'other half' of the population of a great city, the poor and the needy, the underpaid and the underfed, are almost all foreign born, and it is from this class of course that we expect to fill our jails. It was not, however, his birth, but his poverty, that caused the immigrant to commit crime. We do not expect to find criminals so frequently on Fifth Avenue as on the Bowery.

Hunger may prompt a poor starving wretch to steal, and his act is a criminal offense duly recorded, but when greed of money or position or fondness for good living makes the comparatively rich man commit a 'breach of trust,' his error does not appear in prison statistics. Pauperism is the result of the absence of one or both of the prime requisites of a desirable immigrant. The pauper is either unwilling to work or lacks the physique to stand hard labor. The amount of money brought does not affect greatly an immigrant's chances of becoming a pauper. One immigrant may have little money, but with a rugged physique and willingness to work he will not become a public charge; another immigrant, too lazy for laboring work, or

not physically strong, but who is admitted because he claims to be skilled in a trade requiring no great physical strength, may have considerable money and yet be almost certain to become sooner or later dependent upon some one.

There remains the question of race degeneration. On this score the coming of the German, the Irishman, or the Scandinavian no longer causes apprehension. He has demonstrated his capability of making a useful and permanent citizen. His children have attended American schools, and they enter upon life with American ideals in their heads and American patriotism in their hearts. But, as was said before, these races no longer constitute the majority of the immigrants. It is a question whether the Italian, the Jew, or the Slav is willing to merge his identity in that of the nation, and whether such a fusion would not degenerate our race. In order not to deteriorate a race the new elements blended with it should be at least as physically strong as the native race itself.

Some idea of the relative physical condition of the different races from southern and central Europe can be gleaned from the following tables:

TABLE 1.

Ratio of Immigrants by Race requiring Hospital Treatment on Arrival to the Number of that Race Landed during the Fiscal Year ended June 30, 1901:

Race.	Ratio.	Total number of Arrivals.
Slav (Pole, Slovak, Croatian)	1 in every 300	90,888
Magyar	1 " 250	13,311
Finn	1 " 200	9,999
Italian	1 " 185	137,807
Lithuanian	1 " 140	8,815
Hebrew	1 " 100	58,098
Syrian	1 " 35	4,064

TABLE 2.

Ratio of Immigrants Certified as having some Disability grave enough to make them Public Charges, or Dependent upon some one, by Race to the Total Number Landed during the Month of May, 1901:

Lithuanian	1 in every 1,906
Slav (Pole, Slovak, Croat)	1 " 664
Italian	1 " 172
Finn	1 " 163
Magyar	1 " 148
Hebrew	1 " 42
Syrian	1 " 29

The average immigrants are of course not only far below the average American in intellect, but are physically inferior. Sensible expectation does not look for race improvement, but rather that before the new element is assimilated the improved surroundings, better food and cleaner habitations will build up physically the immigrant parent

TABLE 3.

Ratio of Minor Defects in each Race to Number Landed. These were Defects or Conditions not grave enough for Actual Rejection, yet likely to Affect the Immigrant's Chances in making a Living or Usefulness as an Immigrant, such as Poor Physique, Anæmia, Loss of an Eye or Finger, etc. Month of June, 1900:

Finn	1 in every	81
Slav (Croat, Pole, Slovak)	1 "	65
Lithuanian	1 "	64
Magyar	1 "	40
Italian	1 "	26
Syrian	1 "	24
Hebrew	1 "	16

stock whose children, brought up in American schools and in American environment can be assimilated without detriment to the native race. It is not the assimilation of the immigrant but of his children we have to consider.

The American's distrust of the immigrant at present is rational and natural. It is the logical sequence of the sweat shop, the increase of crime and pauperism in New York and other great cities, and of the assassin's act at Buffalo. The percentage of undesirable immigrants is no doubt higher to-day than it was twenty, thirty or forty years ago. It may even be admitted that it has been growing higher year by year during the past two decades. But we are no longer unprotected against undesirable immigrants. Restrictions on admittance have been growing more stringent year by year and a great system has been perfected on Ellis Island for sifting the grain from the chaff, that is doing splendid work, not as a dam to keep out good and bad alike, but as a sieve fine enough in the mesh to keep out the diseased, the pauper and the criminal while admitting the immigrant with two strong arms, a sound body and a stout heart.

Amendment of the present immigration laws has been suggested in some important particulars. It has been urged by a great many people that an educational requirement should be added to the law, barring all immigrants who are illiterate. A glance at the following table will show that this restriction would debar many thousands of our most desirable immigrants—would, in fact, be felt most by the races furnishing us with nearly all of the unskilled labor necessary for our industrial progress. It would not, on the other hand, act as a barrier to some of the least desirable immigrants we receive. The passing of this educational amendment would have one good result: It would lessen the total number of immigrants landed and thus permit of an even more rigid examination of the immigrants upon arrival.

Other remedies suggested for the improvement of the immigration laws are raising the head tax and increasing the time of the Govern-

In the following Table by Illiterates is Meant those who are unable to Read and Write in some Language:

Race.	Total number landed year ended June 30, 1901.	Percentage of illiterates.
Italians	137,807	51.26
Lithuanians	8,815	44.85
Slavs	90,888	34.07
Hebrews	58,098	23.31
Magyars	13,311	7.04
Finn	9,999	1.84

ment's jurisdiction over all landed immigrants and increasing the period of liability of the steamship company for return of undesirable immigrants from one year (the present period) to five years. Increasing the head tax from one dollar, the amount imposed at present upon each arriving alien, to five or ten dollars would probably lessen the number of immigrant arrivals. This reduction in number would be due to the fact that many large families, with children and aged dependents, would be obliged to stay in Europe; the extra five or ten dollars per head in a large family would be sufficient in many cases to make the cost of passage prohibitive. Young single men would pay $40 for passage to America almost as readily as $30; consequently the additional head tax would not greatly affect the number of single unskilled laborers. The young laborer would not go back and forth quite so often perhaps if he had to pay the additional five or ten dollars upon landing here each time.

The period of Government jurisdiction over the landed alien and the period of the liability of the steamship companies for return of undesirable aliens for cause should be extended from one year to five years, or better, until the immigrant becomes a citizen. This would enable the immigration authorities to deport within five years after landing many diseased, insane and pauper immigrants, anarchists and other criminals whose undesirability was not manifest upon landing or within one year thereafter. Relatives and interested persons would not be so ready to offer to the boards of special inquiry a guarantee that detained aliens would not become public charges if such guarantee were binding for five years instead of one year, as at present.

The enactment of these amendments into law and the enactment of stringent legislation bearing upon anarchists, together with a rigid enforcement of our naturalization laws, would go far toward dissipating the present popular distrust of the immigrant, and would still further minimize the dangers due to immigration.

VARIATION IN MAN AND WOMAN.

By HAVELOCK ELLIS.

ARE variations more common in males than in females? That is a question which has passed through various phases during the past century. John Hunter, who touched on the matter from a biological standpoint, vaguely indicated that males are more variable than females. Meckel, on the contrary, came to the conclusion, on pathological grounds, that in the human species females show a greater degree of variability, and he thought that since man is the superior animal and variation a sign of inferiority the conclusion was justified. "We may state as a principle," Meckel wrote ninety years ago at the outset of his manual of descriptive and pathological anatomy, "that anomalies are more common in the female. This phenomenon seems to depend on the eighth law [Meckel's 'law of development,' according to which woman is more primitive than man] since the organization of the female results from development being arrested at an inferior degree." But while he regards deviations as on the whole more common in woman he admits certain exceptions, and more especially instances the heart and the bladder as more variable in man.

Meckel was a profound student of anatomy, but not a very luminous thinker. Some years later Burdach took up the question in his 'Physiologie.' That great biologist at once raised the problem to a higher level, realized its wider bearings and cleared away the prejudices which had surrounded it. He recognized that in some respects women are more variable than men, but pointed out that, contrary to Meckel's opinion, this was no indication of woman's organic inferiority. He showed from the statistics of the Anatomical Institute of Königsberg that we must distinguish between different kinds of abnormality. Further he referred to the facts that indicate that woman is more childlike than man, but, he added, "it is a very common but a very gross error to consider age as a scale of perfection and to regard the child as absolutely imperfect as compared to the adult. It is not imperfection but simply certain childlike characteristics which women preserve"; and, he points out, it is in decrepitude that women take on the characteristics of the so-called superior sex. His general conclusion was that the nature of man and the nature of woman are both excellent, but there are wider variations in men, more genius and more idiocy, more virtue and more vice.

Darwin turned his attention towards this point and accumulated data. In the 'Descent of Man' he brought together many of the chief facts then known concerning variation in man and woman. All the evidence that he could find pointed in the same direction, and he concluded (Part II., Ch. 8) that there is a 'greater general variability in the male sex.'

Some twenty years later in a summary study of human secondary sexual characters entitled 'Man and Woman,' written as a brief introduction to a more elaborate study of the sexual instinct in man, I devoted a chapter to this question, dealing with it more comprehensively than had previously been done and drawing data from a much wider field, but finding no reason to differ fundamentally from the conclusions of Hunter, Burdach and Darwin. I could not indeed assert that as regards man the greater variability of the male is 'general,' but all the facts available since Darwin's day indicated that a greater variability of the male occurred in the majority of the groups of data investigated. And when I considered that this greater organic variational tendency of men is apparently true of psychic variations also—of genius, of idiocy and other mental anomalies having an organic basis—it seemed to me that in the greater variational tendency of man we are in the presence of a fact that has social and practical consequences of the widest significance, a fact which has affected the whole of our human civilization. Although the greater variational tendency of men is balanced by the more equable level of women, we have to recognize that the existence of the exceptional men who have largely created the lines of our progress is based on natural law. It is a conclusion which does not yet appear to me to be fundamentally affected.

There was, however, one important omission in my statement of this question, and I wish to emphasize the importance of the omission because its significance will subsequently become apparent to the reader. I said little or nothing as to the variability of men and women in *size,* either as regards total stature and weight, or the dimensions of parts of the body.* The reason for that omission is clearly indicated in various parts of the volume and we shall encounter it in due course.

Three years later, in a volume of miscellaneous essays entitled 'The Chances of Death,' Professor Karl Pearson published a lengthy paper entitled 'Variation in Man and Woman.' This writer started with the assertion that in 'Man and Woman' I had 'done much

* I did not consider that such evidence must be absolutely rejected—I admitted it in one or two cases (printed in smaller type)—but simply that as it was liable to a discount of unknown extent it could not be placed in the first rank of evidence.

to perpetuate some of the worst of the pseudo-scientific superstitions, notably that of the greater variability of the male human being,' and that it was the object of his essay 'to lay the axe to the root of this pseudo-scientific superstition.' In fact, as he is careful to tell us at frequent intervals, before he himself entered the field (a field, be it remembered, occupied by some of the world's greatest biologists) all was 'dogma,' 'superstition,' 'nearly all partisan,' at the best 'quite unproven,' I am inclined to think that these terms, which spring so easily to Mr. Pearson's pen, are automatic reminiscences of the ancient controversies he has waged with theologians and metaphysicians. They are certainly a little out of place on the present occasion.

In selecting the material for his demonstration, Professor Pearson tells us he sought to eliminate all those 'organs or characteristics which are themselves characteristic of sex,' such being, in his opinion, gout and color-blindness; he also threw aside all variations which can be regarded as 'pathological,' on the hypothetical ground that such 'pathological' variations may have a totally different sexual distribution from 'normal' variations. He decided that size is the best criterion of variability. As to how a 'variation' may be defined Professor Pearson makes no critical inquiry, though such inquiry would very seriously have modified his final conclusions.*

"What we have to do," he states, "is to take healthy normal populations of men and women, and in these populations measure the size of organs which do not appear to be secondary sexual characters, or from which the sexual character can be eliminated by dealing solely with ratios." Various kinds of size are therefore selected for treatment, such as that of the skull, chiefly as regards its capacity and length-breadth index, stature, span, chest-girth, weight of body and of various internal organs, etc., all these, it is observed, being various aspects of the one factor of size. It is shown by careful treatment of the available data—the so-called coefficient of variation being accepted as a possible or indeed probable measure of significant variation—that, as far as there is any difference at all, women are, on the whole, slightly more variable than men. Having reached this result the author leaps bravely to the conclusion, that 'accordingly, the principle that man is more variable than woman must be put on one side as a pseudo-scientific superstition.'

* It is true, indeed, that Mr. Pearson remarks that the question 'What are the most suitable organs or characteristics for measuring the relative variability of man and woman?' 'really involves a definition of variability.' But he adds that 'the definition given may be so vague as to beg off-hand the solution of the problem we propose to discuss.' That suspicion, as we shall see, is not altogether unjustified.

If a reply has so far not been forthcoming from the writer against whom this elaborate paper was chiefly directed, this has not been either because I admitted the justice of its conclusions, or complacently accepted a damnation to which I had been consigned in very excellent company. The subject lay only on the outskirts of my own field; I could claim no originality in it; all that I had done was to sift and bring to a focus data which had hitherto been scattered, and to show their significance. At the same time I again placed the subject on my *agenda* paper for reconsideration. In the meanwhile, it scarcely appeared that Mr. Pearson's arguments met with much acceptance, even from those whom they most concerned.*

Almost the only attempt to consider them, indeed, which I have met, is in a review of 'The Chances of Death' by Professor W. F. R. Weldon, in *Natural Science*. This sympathetic critic, with a biologist's instincts, clearly felt that there was something wrong with Mr. Pearson's triumphant demonstration, although as the subject lay outside his own department he was not able to indicate the chief flaws.

There is indeed one initial flaw in Professor Pearson's argument, to which Professor Weldon called attention; it could scarcely fail to attract the notice of a biologist. We are told that we must put aside 'characteristics which are themselves characteristics of sex,' like gout and color-blindness, which 'without being confined to one sex' are yet peculiarly frequent in one sex. Thus, we see, characteristics not confined to one sex may yet be characteristic of one sex, and when we seek to find what characters vary more in one sex than in the other, we must carefully leave out of account all these characters which are most clearly more prevalent in one sex. Professor Pearson thus sets out with an initial confusion which is never cleared up. His object, he tells us, is to seek such degrees of variability as are 'secondary sexual characters of human beings,' and we infer from the course of his argument that the desired characters while not confined to one sex must yet be peculiarly frequent in one sex. *Yet these are precisely the group of characters ruled inadmissible at the outset!* No definition of secondary sexual characters is anywhere given, or on such premises could be given.

Professor Pearson seems to assume that the conception of a sec-

* Mr. Pearson has endeavored to find an opponent of the greater variational tendency of men in Tennyson, who wrote:

> For men at most differ as Heaven and earth,
> But women, worst and best, as Heaven and Hell.

This argument, however—whatever it may be worth—had already been answered by anticipation in a chapter of 'Man and Woman' on the affectability of woman in which I pointed out that the 'Heaven and Hell' of woman are both aspects of her greater affectability; not only does one woman differ from another as 'Heaven and Hell' but the *same* woman may so vary at different times.

ondary sexual character is too obvious to need definition. As a matter of fact there is considerable difference of opinion. Since Hunter first spoke of the 'secondary properties' of sex, which he regarded as dependent on the primary, only developing at puberty, and principally, though not entirely, confined to the male, the conception has very much changed; there has been a tendency to throw all sorts of miscellaneous sexual differences into the category. I have suggested that it would be convenient to introduce a group of 'tertiary' sexual characters, keeping the term 'secondary' to its original sense and reserving as 'tertiary' all those minor differences which are not obvious, which can have no direct influence on mating, and only exist as averages; such are the composition of the blood and the shape of the bones.*

It is difficult to correct all the errors and confusions which Professor Pearson falls into at this point. He remarks that we must not regard the greater prevalence of idiocy among men as evidence of greater male variability, unless we count on the other side the greater prevalence of insanity among women. The error here is double. As a matter of fact, although in England and Wales during recent years the incidence of insanity has been as great on women as on men,† nearly everywhere else it is markedly greater in the case of men. Indeed even in England and Wales, at the present time, if we may trust the Commissioners in Lunacy in their latest annual report (1902), the incidence of insanity as indicated by the admissions to asylums, is, *in ratio to the male population,* still slightly greater in the case of men. Professor Pearson has been misled by the greater accumulation of females in asylums, failing to take into consideration the greater longevity of women, which among the insane is specially marked.‡ But even if the facts had been as stated by

* Professor Waldeyer, who has done me the honor of critically examining some of the main points in my book (in the form of an address at one of the annual meetings of the German Anthropological Society) is inclined to doubt the value of this distinction since there is no clear line of demarcation between secondary and tertiary characters. That, however, I had myself pointed out, and the objection cannot logically be held by any one who accepts secondary sexual characters and recognizes that they merge into the primary. There are many natural groups which have nuclei but no definite boundary walls.

† It so happens that I was the first to call attention to the fact that towards the end of the last century the number of women admitted to asylums in England and Wales had for the first time begun to exceed the number of men. (Art: 'Influence of Sex on Insanity,' in Tuke's 'Dictionary of Psychological Medicine.')

‡ How serious this fallacy is may be indicated by an illustration that chances to come to hand almost as I write. I read in a South American medical journal that in the Asylum of Santiago in Chili on the 1st of January, 1901, there were 560 men and 655 women, but, during the year, 539 men were admitted and only 351 women.

Mr. Pearson, his inference would still have been wrong; idiocy is mainly a congenital condition and therefore a fairly good test of organic variational tendency; insanity, though usually on a hereditary basis, is invariably an acquired condition, dependent on all sorts of environmental influences, so that it can not possibly furnish an equally fundamental test. Color-blindness, Mr. Pearson also tells us, is a peculiarly male 'disease,' and must not be used as an argument for greater variability in men unless we use the prevalence of cancer of the breast in women on the other side. Again there is a double error; not only is a congenital anomaly improperly compared to an acquired disease, but a gland like the breast which is only functional in one sex is paired off with an organ like the eye which is equally functional in both sexes. The prevalence of gout among men is, again, paired off against the prevalence of hysteria among women. Here the error is still more complex. Not only is gout not a truly congenital condition, though, like insanity, it frequently has a hereditary basis, but if we take into account conditions of 'suppressed' gout it is by no means more prevalent in men than in women, and even if we do not take such conditions into account, it is still not possible to pair off gout against hysteria, since, although in some countries hysteria is more prevalent in women, in others (as, according to some of the best authorities, in France) it is found more prevalent in men. But it would be tedious to explore further this confused jungle of misstatements.

From the point of view of sexual differences in variational tendency it is not necessary to exclude rigidly either 'tertiary,' 'secondary,' or even 'primary' sexual characters, provided we are careful to avoid fallacies which are fairly obvious, and do not compare organs and characters which are not truly comparable. Even those secondary sexual characters which are almost or entirely confined to one sex may properly be allowed a certain amount of weight as evidence, especially if we grant that such characters are merely the perpetuation of congenital variations. If, therefore, as is generally agreed, such characters more often occur in males, that fact is a presumption on the side of a greater male variational tendency which there is no reason entirely to ignore. It is not conclusive, but it must receive its due weight. To assume, with Professor Pearson, that a variation has no variational significance because it occurs often in one sex and seldom in the other* seems altogether unwarrantable.

If, however, Professor Pearson's attempt to discriminate between different kinds of sexual characters from the point of view of sexual

* In the case of ordinary gout, which Professor Pearson regards as typical of the tests to be excluded, opinions differ considerably as to sexual liability; according to one leading authority it is 68 men to 12 women.

variability fails to work out, and is in any case unnecessary, at another point he falls into the opposite mistake of making no attempt to discriminate when discrimination is of the first importance. As we have already incidentally seen, it seems to him to be of no importance whether the variational tendency is tested by variations having an organic congenital base, or by variations which may be merely due to environmental influences during life.* To him they are all alike 'variations,' and the most important are those that can most conveniently be caught in the mathematical net. Indeed he goes further than this. He actually discriminates *against* the more organic and fundamental kinds of variation. It seems to him 'erroneous' to take into account congenital abnormalities of any kind when we wish to test the relative variability of the sexes. In determining the variational tendencies of the sexes we must leave out of account the majority of variations!

The ground on which Professor Pearson rejects abnormalities is that they are 'pathological,' and that it is conceivable that pathological variation might be greater and normal variation less in the same sex.† He believes that in regarding the 'normal' and the 'abnormal' as two altogether different and possibly opposed groups of phenomena he is warranted by 'current medical science.'

This is very far indeed from being the case. It is quite true that in ordinary clinical work the physician does make such a distinction; it is practically convenient. But it is not science, and if the physician is a genuine pathologist he admits that it is not. This is so well recognized that I had thought it sufficient to quote the remark of the greatest of pathologists, Virchow, to the effect that every deviation from the parental type has its foundation in a pathological accident—a statement which Professor Pearson, on the strength of what is really a verbal quibble, contemptuously puts aside as 'meaningless.' We ought not to say the 'parental type,' he tells us, we ought to say 'a type lying between the parental type and the race type'; let us say it—and the statement remains substantially the same, so far as the question before us is concerned.‡

* It is scarcely necessary to remark that the two groups cannot be absolutely separated.

† This conception, Mr. Pearson remarks, seems never to have occurred to me. In that shape, happily, it has not. But in 'Man and Woman' and elsewhere I have repeatedly called attention to the fact that, as regards various psychic and nervous conditions, while gross variations are more frequent in men, minor variations are more common in women. This seems to cover whatever truth there may be in Mr. Pearson's supposition.

‡ Virchow repeatedly emphasized the statement in question and by no means always in the form that offends Professor Pearson. Thus he remarked in 1894, at the annual meeting of the German Anthropological Society, that

Virchow is by no means the only pathologist of high authority who has distinctly laid down this principle. Thus, as Ballantyne points out—when remarking that the ancient belief, held even by Simpson, that anomalies and malformations are due to disease has been supplemented by modern research—Mathias Duval has emphatically declared that it is not to be thought that the malformation of any part is a result of disease of that part.*

Even, however, if we go back to the time of Simpson, and earlier, we find that Meckel—who is sometimes regarded as one of the founders of the study of variations—clearly recognized that the simplest anomalies and varieties pass gradually into monstrosities, and that the same laws apply to both.†

Indeed so did Hunter in the previous century. 'Every deviation,' he wrote at the outset of his almost epoch-marking 'Account of an Extraordinary Pheasant,' 'may not improperly be called monstrous,' so that 'the variety of monsters will be almost infinite.'

The tendency of scientific pathology is at once to push the frontiers of the normal into regions popularly regarded as belonging to disease, and at the same time, when actual disease comes into question, to refuse to admit that any new laws are brought into operation. "Between any form of disease and health," one of the founders of modern pathology declared a quarter of a century ago, "there are only differences of degree. No disease is anything more than an exaggeration or disproportion or disharmony of normal phenomena."‡ The notion that disease and health are distinct principles or entities Bernard regarded as a sort of idea belonging to the medical lumber

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whenever 'the physiological norm hitherto subsisting is changed' we are in the presence of an anomaly and that in this sense every departure from the norm is a pathological event, though it is not a disease and may not be harmful, may even be advantageous. In what was perhaps his last utterance on the subject (*Zeitschrift für Ethnologie*, 1901, p. 213) he repeated that pathology as well as physiology is an essential factor in the development of the human race. Pathologists will, I know, agree with me that a conviction of the essential unity of physiology and pathology lay at the foundation of the pathological revolution which Virchow effected.

* With this result Dr. Ballantyne—who may be said to be the chief British authority on pathology in its antenatal aspects—in the main concurs. He even goes so far as to assert ('Manual of Antenatal Pathology,' 1902, p. 35) that natural birth in its effects on the child may almost be regarded as a pathological process; 'it is very certain that the same amount of distortion of parts, occurring at a later period of life, would be termed pathological.'

† 'Handbuch der pathologischen Anatomie,' 1812, Vol. I., p. 9.

‡ Claude Bernard, 'Leçons sur la chaleur animale' (19th Lesson), 1875.

room. These conceptions have been brilliantly developed in the work of recent pathologists.*

And if it is argued that a mathematician cannot be supposed familiar with the principles of pathology, it must be replied that Mr. Pearson has here ventured along a path which leads immediately up to these principles, and, further, that the principle in question is so simple and elementary that it may already be said to have entered general culture. I take up the latest volume of Nietzsche's works ('Der Wille zur Macht')—written more than ten years ago, though only now published—and read: "The value of all morbid conditions is that they show us in magnified form certain conditions that are normal, but in the normal condition not easily visible. . . . *Health* and *disease* are not essentially different, as the old physicians and some modern practitioners have believed. To regard them as distinct principles struggling for the living organism is foolish nonsense and chatter."

On the whole, then, there is no reason for rejecting abnormalities when we are considering the relative variational tendencies of men and women. To the mathematical mind—Professor Pearson forces us to admit—it is possible to conceive that the laws of pathology may reverse the laws of physiology, but such a conception the biologist regards as absurd.

More than this must, however, be said. Not only can we not leave anomalies out of account in dealing with this question, but it is precisely the anomalies which furnish us with the most reliable evidence. The word 'abnormality' is apt to mislead, and Professor Pearson somewhat prejudices the matter in unscientific ears by insisting on its use. It is not a scientific term; the so-called anomaly is not abnormal in the sense that it is morbid; it is only exceptional. It merely indicates the extreme swings of a pendulum whose more frequent oscillations are popularly regarded as 'normal.' What is commonly termed an 'anomaly' might really be regarded as the 'variation' *par excellence*.

Such an assertion would be by no means arbitrary. It does in fact correspond with the usage of most of the writers who have investigated this matter until the present day, and it is possible to justify such usage. If—to return to the image of the pendulum—we wish to find out whether the male or female pendulums swing farthest, we must so far as possible let them swing freely; the more they are restrained by external forces the less the significance of the results we

* To those who may wish to gain an attractive insight into modern conceptions of pathology—according to which disease is a relative term and its study a branch of biology—I would recommend Professor Woods Hutchinson's highly suggestive 'Studies in Human and Comparative Pathology' (1901).

reach. Now the congenital 'anomalies' are precisely the kind of variation that most nearly corresponds to the free swing of the pendulum. It is true that there is no absolute distinction between the initial energy and the subsequent modifying influences, but it is equally true that if we wish to measure and compare the aboriginal energies of the male and female organisms, we must so far as possible disregard those characters which are very considerably influenced by late modifying forces.

Professor Pearson has, however, chosen, as a final and crucial test of the variational tendency in men and women, the single point of difference in size, chiefly in adults. That is to say, he has selected as a final and unimpeachable test one of the most fragile of distinctions, a distinction that has been exposed to a lifetime of modifying influences that are incalculable.

Even if we admit that size at birth constitutes a sound test—and this can not be admitted without qualification, as we shall soon see—it is evident that the comparative variation of the sexes in this respect is liable to be affected by environmental circumstances as age increases. The influences of life differently affecting and exercising the two sexes, the influence of death probably exerting an unequal selective influence—both alike must be allowed for if this kind of evidence is to be regarded as a test of the first rank of importance. Otherwise we are not dealing with the incidence of variations at all, but with the elimination of variations—an altogether different matter. Professor Pearson himself gradually awakes to a realization of this fact as he proceeds with his task, and remarks at last that he strongly suspects that the slightly greater variability of woman which his results show is mainly due to a relatively less severe struggle for existence! Probably he is right, but if so his whole argument falls to the ground. The question of the organic variational tendencies of men and women remains untouched; we have been introduced instead to a problem in selection. So true is it that, as Bacon said, the half of knowledge lies in asking the right question.

We are bound to suppose that when Professor Pearson set out he intended to use the term 'variation' in the same sense as his predecessors had used it—for otherwise his results could not validly be opposed to theirs—but it would appear that as he went on, by an unconscious process of auto-suggestion, he insensibly glided into a familiar field.*

It may seem unnecessary to pursue Professor Pearson any further. It is sufficiently clear that the inquiry he has carried out, however

* It is perhaps significant that while I had dealt with the 'organic variational tendency' Mr. Pearson prefers the vaguer term 'variability,' which enables him to bring in such matters as strength of pull and squeezes of hand, which are merely due to functional exercise.

valuable it may be in other respects, has no decisive bearing on the question he undertook to answer, and can have no very damaging effect on the writers he attacks. But there is considerable interest in driving the point of the discussion still further home.

It may be agreed that since differences in size are probably affected by the influences of life and death to a considerable extent, and perhaps unequally eliminated in the two sexes, they do not form a reliable guide to the sexual incidence of variations. But, it may be argued, this cannot affect measurements made at birth, and we must therefore accept the validity of those of Professor Pearson's measurements which concern the infant at birth. Here, however, we encounter a fact which is of the first importance in its bearing on our subject: the elimination of variations in size has already begun at birth, and there is reason to suppose that that elimination unequally affects males and females. This was duly allowed for in 'Man and Woman,' but there is no hint of it throughout Professor Pearson's long paper. He does not dispute this influence, nor does he realize that until he has disputed it his conclusions can not be brought to bear against mine. Professor Pearson's earlier statistical excursions into the biological field were chiefly concerned with crabs; in passing from crabs to human beings he failed to allow for the fact that human beings do not come into the world under the same conditions. I make no large claim for superior insight in this matter; it was probably a question of training; I was practically familiar with the phenomena of childbirth; he was not. But his ignorance has profoundly affected the validity of his cherished criterion of sex variability, in so far as it is used against his predecessors in this field.

Every child who is born into the world undergoes a severe ordeal, due largely to the limited elasticity of the bony pelvic ring through which it has to pass. Probably as a result of this, a certain proportion perish as they enter the world or very shortly after. Among the number thus eliminated there appears to be a very considerable proportion of the largest infants. Doubtless because male infants tend to be larger than female infants, males suffer most at and shortly after birth. This appears to be the rule everywhere.*

So far as I am aware, the first attempt to explain this matter scientifically was made in 1786 by an English doctor named Clarke, physician to the Lying-in Hospital at Dublin.† By weighing and

* For the exact proportion of male to female still-born children in most civilized countries, see, *e. g.*, Ploss, 'Das Weib,' 7th edition, 1901, Vol. I., p. 336.

† Joseph Clarke, 'Observations on some Causes of the Excess of the Mortality of Males above that of Females,' *Philosophical Transactions*, 1786. It may be said here that the very first attempt to weigh and measure infants accurately had only been made not so many years previously, by Roederer, in 1753.

measuring 120 newborn infants of both sexes he found that there was a marked tendency for the males to be larger than the females. 'Hence appears,' as he is pleased to put it, 'the merciful dispensations of Providence towards the female sex, for when deviations from the medium standard occur it is remarkable that they are much more frequently below than above this standard.' He considered that the greater mortality of males at and shortly after birth is largely due to the injuries to the head occurring at birth, but also that, since the males are larger and therefore make from the first a larger demand on the nutritive capacity of the mother, they are more likely to suffer from any defect of the mother in this respect. The problem and its possible and probable explanations were thus clearly stated more than a century ago.

As often happens with pioneers, Clarke's little paper was forgotten, and for more than half a century, although a number of workers brought extensive contributions of new data, their attitude was frequently illogical or one-sided, and the progress of scientific knowledge was not great. In 1844 Simpson published a well-known study which brought together a mass of evidence bearing more or less on the question before us. He showed that in male births the mothers suffered excessively as well as the infants; he refused to admit that the greater mortality of males at and shortly after birth could be due to any other cause than the generally recognized larger size of the male head (mainly on the ground that fœtal deaths up to birth are fairly apportioned to the two sexes) and concluded that the greater size of the male head is the cause of a vast annual mortality. A number of later obstetrical inquirers furnished additional contributions to the matter, at one point or another, though not always agreeing that so great a mortality could be due to a difference of size which seemed so small. One authority, indeed, roundly declared that the belief in the larger size of the male head was merely 'a popular prejudice'; this led to fresh measurements, and in this field Stadtfeldt of Copenhagen received credit which really belonged to Clarke of Dublin. Veit showed that even at equal weights more boys than girls die at birth, but, on the other hand, according to Pfannkuch's results, even at equal weights boys' heads are larger than girls'. In any case it certainly seemed probable that the larger size of the male child's head was an important factor in this mortality, and when at length the question began to attract the attention of statistical anthropologists this conclusion was confirmed. The Anthropometric Committee of the British Association, presided over by Mr. Francis Galton, in its final report in 1883 stated its belief that "it would appear that the physical (and most probably the mental) proportions of a race, and their uniformity within certain limits, are largely dependent on

the size of the female pelvis, which acts as a gauge, as it were, of the race, and eliminates the largest infants, especially those with large heads (and presumably more brains), by preventing their survival at birth."

It must be added, however, that no direct and final demonstration has been brought forward of the tendency to the elimination of the males (or even infants independently of sex) of the greatest weight or those having the largest heads. For this we require to compare male and female stillborn infants at full term with those who are born living and which subsequently survived for at least a week (a longer period would be more desirable but difficult to secure). Such measurements are not to be found in medical literature, so far as I can discover; at the most we find averages, which are meaningless from the present point of view. I applied to obstetrical and anatomical authorities in various countries and received a number of interesting letters and data, including series of entries from the registers of maternity hospitals. But none of the series so far received contains a sufficient number of stillborn children. So far as they go, they are confirmatory of the belief that it is more especially the large children that are eliminated by the selection of birth. The largest series (with 60 stillborn male babies and 50 stillborn females), for which I am indebted to Dr. C. M. Green, of the Boston Lying-in Hospital, shows that among the stillborn of either sex the range of variation is greater than among the living of the same sex, the absolute range of variation being not only greater as compared with the living babies of the same sex, but there being a greater piling up at each end in the case of the stillborn. The data do not suffice to indicate that there is a greater mortality of the largest sized males than of the largest sized females, when we compare the stillborn with the living of the same sex and weight. Another series, more elaborate in its details but still smaller in number as regards the stillborn—for which I am indebted to Professor Whitridge Williams, of Johns Hopkins Hospital —leads to a similarly incomplete conclusion. I still await more extensive data which have been promised me from a British source.*

There is, however, another test which, while it can by no means be put forward as having any statistical validity, yet furnishes a highly significant indication in this matter. Just as on the psychic side certain very rare individuals appear in the world whose intellectual capacity enormously excels that of their fellows, so, corresponding to

* It seems unnecessary to deal with this point more in detail, not only because of the lack of sufficient data but because the establishment of this point is not necessary for the criticism of Professor Pearson's position. I expect to return to the point elsewhere, and hope that others, who may be more fortunate than I am in obtaining extensive data, will be able to deal with it on the lines I suggest.

'genius,' we have on the physical side certain equally rare individuals who at birth enormously excel their fellows in physical size, while yet remaining normal and well proportioned. Now, we may ask, do these individuals possessing congenital physical 'genius' resemble persons of psychic genius in being more often male than female? Ordinary statistics are not here available, for these cases are so rare that they very seldom fall into an ordinary series. Smellic found one child weighing over 13 pounds in 8,000 cases; in France, a child of 12 pounds was only found in 20,000 cases.* As even a child of 9 pounds is generally considered large, it is clear that when we get beyond 13 pounds we reach a point at which the average difference between males and females is trifling, so that there is almost as great a chance of females as of males reaching the extremely large weights. The only practicable way of obtaining information concerning these cases lay in collecting the scattered records. I have collected all that I can find in medical journals of standing, chiefly English, during the past half century, being aided by the references in Neale's 'Medical Digest.' I have only noted the cases that appear to have been healthy and well developed and weighed over 13 pounds at birth. One unexpected difficulty I encountered: in many cases, even when numerous measurements were given, no reference was made to sex. While such cases were necessarily rejected, I may say that I think it probable that most, and perhaps all, of these rejected cases were males; this was so in the only case in which, by writing to the medical reporter immediately on publication, I was able to repair the omission; the medical mind seems to share in some degree the instinctive conviction that the typical human being is a male, and that in the case of males it is unnecessary to make any reference to sex. My cases were thus reduced to 21. Of these there were only 3 females to 18 males. The females all died at birth, as well as about half the males. However rough this method of estimation may be, it is highly improbable that any more methodical inquiry on children of this size would entirely reverse so large a preponderance of males.

Such a result, it will be seen, can not be considered as absolutely conclusive proof that there exists a selection of birth which in its operation tends to the destruction of the larger male children either at the moment of birth or during the succeeding days and weeks, though it renders such selection probable. This element of doubt, however, by no means makes Professor Pearson's position any stronger. It

* It must also be said that (as in the case of psychic genius) it is among the well-to-do classes that these very large infants are most usually found, not only because the parents tend to be larger among these classes, but because, as has lately been shown, other things being equal, women who rest during pregnancy tend to have larger children.

is sufficient to show that for more than a century past evidence has accumulated which indicates that the group of data on which Professor Pearson solely and absolutely relies for the foundation of his argument is modified by an influence which renders it tainted for such a purpose. In view of this circumstance, and of the fact that I had rejected this group of evidence on these grounds, the *onus probandi* clearly rested with Professor Pearson. In other words, he had to show either that male children are not larger than female children at birth, or else that large children do not suffer more than smaller children in passing through the maternal pelvis. The fact that Professor Pearson gives no indication that he had realized the necessity of this preliminary step is sufficient proof that he was not adequately equipped for the task he has undertaken.

We now come to a point which is not the less interesting for being entirely hidden from Mr. Pearson. It has been seen that the selection exercised by the pelvis to the detriment of male children is not absolutely proved. But if for the moment we assume that it exists, what are the phenomena that we should expect to find, as regards size, among the survivors? Obviously, a more or less diminished sexual difference during life, *with a maximum of sexual difference immediately after birth.** Now this is exactly what Professor Pearson found! 'Summing up in general our conclusions for weight,' he states, 'it would appear that, except at birth, man is not more variable than woman.' The very great significance of this exception, as affecting any argument on these premises brought against the position maintained in 'Man and Woman,' he undoubtedly failed to see. Still the exception evidently puzzled him. He accumulated series of data on the subject, and indeed initiated an entirely new and very extensive investigation. But the conclusion remained on the whole unaffected. Thus we see that our author, in all innocence, supplies a valuable piece of proof in favor of that very position which he imagines that he is upsetting! If this is the way that the axe is to be laid to the root of 'pseudo-scientific superstitions' they will certainly continue to flourish exceedingly.

We have now reached the climax of Professor Pearson's argument. It is from this giddy height that Mr. Pearson surveys with contempt those foolish persons who still believe that the variational tendency is greater in men than in women, and nothing further remains to be said. If instead of hastening to execute a war-dance on what he vainly imagined was the body of a prostrate foe, Mr. Pearson had pointed out, as he would have been quite warranted in doing, that

* In children dying at or soon after birth, as a result of undue pressure, hemorrhages or congestions are nearly always found in the internal organs, but they are not of necessity immediately fatal.

his conclusions, so far as they rested on a definite basis of fact, confirmed the thesis maintained by Darwin and more fully enforced in 'Man and Woman,' his position would have been unimpeachable. If, again, he had refrained altogether from attempting to interpret his own data—a task for which, it is obvious, he was singularly ill-prepared—and had put them forth simply as a study in natural selection —which is what they really are—his position would, again, have been altogether justifiable. But as the matter stands he has enmeshed himself in a tangle of misapprehensions, confusions and errors from which it must be very difficult to extricate him.

It may be well to summarize briefly the main points set forth in the foregoing pages.

1. In opposition to the doctrine of Darwin, more fully set forth in my 'Man and Woman,' that the variational tendency is, on the whole, more marked in men than in women, Professor Pearson resolved to show that this is one of 'the worst of the pseudo-scientific superstitions.'

2. Unfortunately, however, it never occurred to him to define what he meant by 'variation,' nor to ascertain what the writers whom he was opposing meant by the term.* A very little consideration suffices to show that a typical variation, in what may fairly be called its classic sense, is a congenital organic character *on which selection works,* while, as understood by Professor Pearson, though without definite statement, a typical variation is a character—of almost any kind, occurring at any period of life—*produced by selection.* 'To the biometrician,' Professor Pearson has recently stated, 'variation is a quantity determined by the class or group without reference to its ancestry.' That is to say, it need not be organic or congenital, and it must usually be modified, and sometimes entirely produced, by its environment. This definition may be better than the more classical conception of a variation. But it is certainly very different. To suppose that conclusions reached concerning this kind of variation can be used to overthrow conclusions reached concerning the other kind is obviously unreasonable.

3. Having silently adopted this conception of a variation, Professor Pearson proceeds to inquire what 'different degrees of variability are secondary sexual characters' and not 'characteristics which are themselves characteristics of sex'; and is hereby led into various eccentricities of assertion which it is unnecessary to recapitulate.

* It is somewhat unusual, Professor Pearson has remarked in a recent controversial paper ('Biometrika,' April, 1902, p. 323), 'in a discussion to give entirely different meanings to the terms originally used, and leaves your adversary to find out with what significance you may be using them.' It seems to occur sometimes however.

'Secondary sexual characters' remain, in his hands, like variation, undefined.

4. All 'abnormalities' are added to the material rejected as unsuitable for investigation, on the ground that they are 'pathological.' It has been easy to show that this notion cannot be maintained, and that in his pious horror of 'pseudo-scientific superstitions' Professor Pearson here lays himself open to retort. Anomalies are not pathological, except in the sense of Virchow, who regarded pathology as simply the science of anomalies. Moreover, scientific pathologists do not admit that even diseases can be regarded as involving any new or different laws. Morbid as well as normal phenomena alike furnish proper material, if intelligently used, for the investigation of this question.

5. Professor Pearson decides that differences in size furnish the best measure of the variability of the sexes. In reaching this decision he makes no reference to the fact that the probabilities accumulated during a century tended to discredit this group of evidence for the purposes he had in view.

6. If, however, we put aside those probabilities which tend to render this evidence tainted, so far as the object of Professor Pearson's special argument is concerned, we still find that the results he reaches are precisely the results we should expect if the position he assails is sound. That is to say that at birth, before the results of the assumed selective action of the pelvis have yet been fully shown, there is greater variability of the males, while later, as a result of that selection, there is a tendency to equality in sexual variability.

7. The net outcome of Professor Pearson's paper is thus found to be a confirmation of that very doctrine of the greater variational tendency of the male which he set out to prove to be 'either a dogma or a superstition.'

It may be as well to state, finally, that nothing I have said can be construed as an attempt to disparage those 'biometrical' methods of advancing biology of which Professor Pearson is to-day the most brilliant and conspicuous champion. I am not competent to judge of the mathematical validity of such methods, but so far as I am able to follow them I gladly recognize that they constitute a very valuable instrument for biological progress. I say nothing against the instrument: I merely point out that, on this occasion, the results obtained by its application have been wrongly interpreted.

THE ENGINEERING MIND.

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IN a fragment of autobiography written some years before his death, Mr. Huxley said:

> But, though the Institute of Mechanical Engineers would certainly not own me, I am not sure that I have not all along been a sort of mechanical engineer *in partibus infidelium*. I am now occasionally horrified to think how very little I ever knew or cared about medicine as the art of healing. The only part of my professional course which really and deeply interested me was physiology, which is the mechanical engineering of living machines; and notwithstanding that natural science has been my proper business, I am afraid there is very little of the genuine naturalist in me. I never collected anything, and species work was always a burden to me; what I cared for was the architectural and engineering part of the business, the working out the wonderful unity of plan in the thousands and thousands of diverse living constructions, and the modifications of similar apparatuses to serve diverse ends.

Those who have read, however, the intensely interesting 'Life and Letters' of Huxley by his son, will recall that at the very close of his career, when driven to the continent in search of health, he took to collecting gentians and determining their species, with great enthusiasm. The physical enjoyments of the search, as well as the pleasure of recognizing each new species that he ran across, were doubtless added to the more direct pleasure he derived from observing the distribution of the genus and the adaptations that the different species had undergone.

At the same time, Huxley's analysis of the foundation of his intellectual pleasure serves to indicate a special 'note' of modern culture; or rather that part of modern culture which has been most profoundly affected by scientific thought. The age, at its best, is the age of the engineering mind. By this is not meant merely that it is an age of vast engineering feats and of a remarkable development of the engineering profession, but that a distinct habit of thought which may be called both with convenience and propriety the 'engineering mind,' is deeply influencing modern culture and is steadily preparing the way for the realization of a better ideal in popular education. The capable engineer computing to a nicety the elements of his bridge structure, the botanist studying the wonderful mechanism for the dispersal of seeds on the withered autumn weeds, the captain

of industry organizing his factory, the skilled workman himself, the surgeon, the sea-captain—each and all of these, with many other representatives of human activity, are, at their best, endowed or equipped with a common habit of thought more or less directly connected with what Huxley called 'the architectural or engineering part of the business.' In other words, the more or less distinctly pronounced 'note' of modern culture is a capacity for the recognition of the universal in the particular or the reign of law in nature.

Unquestionably, the intellectual training which leads to the formation of the engineering mind, in its larger sense, affords a larger scope and capacity for pure intellectual pleasure, as well as a more permanent source of such pleasure, than is afforded, for instance, by the popular resort to light fiction. The poetry of common things, disclosed in enormous volume by the science of the nineteenth century, is familiar to many; but it is sealed to many more, not without a certain measure of intellectual culture, by the lack of the special training which forms the scientific habit of thought.

But, apart from the value of the engineering habit of mind regarded from the point of view of intellectual culture for its own sake, the question is at last being recognized with widespread interest as one of importance to nations struggling for industrial supremacy or stability. In Great Britain particularly, thanks in part to the large attention it received at the September meeting of the British Association not only at the hands of the president, Professor Dewar, but from the engineering and educational sections, the question is up for very general discussion in the country and, it is to be hoped, for progressive settlement. The incompetence displayed so often by British officers in South Africa has driven the British people to a severe stock taking, and that stock taking has brought into prominence a fact, more or less fully recognized by a wise minority from the time of Dr. Arnold of Rugby to the present day, namely, that the traditional methods of education in England are not conducive to the formation of trained habits of scientific thought.

Professor Perry, the president of the engineering section of the British Association, has been insisting for some time, and insisted again at the Belfast meeting, that the great fault of the traditional method is the manner in which mathematics is taught. Mensuration is dissociated too sharply from geometry, and geometry too sharply from algebra. That is to say, contrary to the example of Germany and France for nearly a century and to the more recent example of the American universities, the employment of the modern proofs of geometry has been resisted in favor of the more cumbersome and, to the average young mind, the far more difficult proofs employed by

Euclid. The amount of geometrical ground that can be covered by means of the algebraical methods in one year by an average pupil is about equal to that which can be acquired by the same pupil in three years following the Euclidean method. And as there is no loss whatever in rigidity of proof, the natural consequence is surely that of two pupils who have devoted an equal period of time to mathematics during their school training, the one employing in his geometry the modern methods and the other the Euclidean, the former must be more prepared for the intelligent use of mathematical formulæ and reasoning in the university or the technical school than the latter. This consideration, indeed, derives still more strength from the fact that the proving of geometrical propositions by means of operations upon algebraical symbols extends the mental grasp of algebra itself.

Professor Perry appears to have hopes of speedy reform in this respect, the University of Oxford having decided to omit Euclid from the 'locals' of 1903, Oxford being capable of setting the pace for the great schools and the principles of the Society for the Improvement of Geometrical Teaching having made, during the past twenty years, sufficient theoretical headway to ensure the opportunity of the change being welcomed and grasped.

At the same time, it is to be remembered that the mathematical is not the whole or the only training even for the engineering mind. We have the constant reminder of Faraday's example in this respect. Faraday was able to reason most accurately and profoundly upon curves, centers of motion and other phenomena arising from his experiments in electricity and magnetism, although he was obliged to confess in a letter to Professor Clerk Maxwell that although he had tried hard he had never been able to understand even simple equations in algebra—a comforting confession for others to whom mathematical studies have been a great difficulty!—but perhaps more profitably to be regarded simply as a proof that a sufficiently helpful text-book was not placed within his reach at the struggling period when he laid the foundations of his marvelous self-culture.

POST-GRADUATE DEGREES IN ABSENTIA.

By A. L. BENEDICT, A.M., M.D.,
BUFFALO.

COLLEGE faculties have, within the last few years, conferred post-graduate degrees with conservatism and even reluctance. The time is long past when there was a germ of truth in the assertion that A.M. was a decoration for principals of preparatory schools who sent a sufficient number of students to college, or for young alumni whose interest in their alma mater persisted after graduation. The free distribution of honorary degrees, always a possible source of evil, is especially dangerous in the case of professional degrees, since the latter indicate the completion of an apprenticeship rather than the attainment of learning and confer privileges of practical commercial value and subject to abuse.

Unfortunately, the reaction against the old custom of applying degrees 'honoris causa' and with no very definite requirement of scholarship, has led the majority of colleges to insist that the master's and the doctor's degree shall be reached only by courses of study pursued under the immediate supervision of the faculty and has closed the path to these honors for all who are unable to protract their residence in a college town, except those who have distinguished themselves in the most signal manner. It is, doubtless, presumptuous for criticism of an educational system to emanate from one who has no closer contact with education than the training of a professional student and the practice of a profession, yet the general tendency of colleges to adapt their methods and aims to conform with the demands of practical life, encourages the writer in pleading in behalf of the worker in the great university of the world, who still desires to keep in touch with the scholarship of the college.

Through long custom, we have one degree which is admirably adapted to use as a decoration. This is the title of Doctor of Laws, which has come to be the patent of practical success in any line of activity, not incompatible with a reasonable degree of refinement and intellect. Its significance is executive ability and wide influence of the highest kind. If deservedly applied, it can never add materially to the dignity of the recipient, while any tendency to its abuse is checked by the reflex discredit cast upon the donor.

The master's and doctor's degrees in arts and sciences, on the other hand, represent purely educational attainments, of higher order

than the bachelor's, but of the same general scope, and they may be sought with the same propriety as any other reward which represents a performance of a definite task and which is honorary only in the sense that the formal recognition of an accomplished work is an honor. The plea for conferring these degrees *in absentia* might, at first thought, be regarded as in the interests of young men and women, prevented by poverty—actual or relative—from pursuing their studies beyond the usual limits of the college course. On the contrary, few are prevented from continuing their education at college by lack of funds, on account of the generous provision of scholarships and because the experience of undergraduate life renders it comparatively easy for the post-graduate student to be self-supporting, as a tutor, a literary hack or in some other capacity. The real obstacle to post-graduate study *in præsentia* is that every young person of energy and ambition realizes, with the advent of that indefinable condition which we call maturity, that it is time for him to be about the serious business of life, that he must cease to be a consumer, even of scholarship, and that he must become a producer. Some few lines of life work admit of a protraction of residence at a university without interference with the demands which society justly makes on a well-trained intellect, some few are favored by accident of location, but, in the vast majority of all instances, the man or woman who decides to remain at college beyond the usual undergraduate period, must make a sacrifice of the best years of life, years which might better be applied to the preliminary struggle for position which is inevitable to success in every business or profession and which must be undertaken in the arena of actual life. The desire for thorough educational preparation, however laudable, must be recognized as futile in the sense that no scholar can hope to gain the point at which he can consider his past progress as having measurably subtracted from the infinite possibility of the future. On the other hand, all educational systems must frankly recognize that senility begins its inroads before full maturity is reached. The appearance of grey hairs before the beard is fully established is but the symbol of all physical and mental development. The man who waits for his judgment to be fully formed and his knowledge to be completed—even according to human standards—before engaging on his life work, has already lost something of mental flexibility and of the vigor of innervating centers. It is impossible to translate this principle into terms of age and the formulation of standards must be left to the collective experience of educators, sociologists and of that paramount factor in education and social progress which we so often forget—the people. A surprisingly large number of great men have practically completed their work in life

at thirty-five. A critical study of most others will demonstrate that, while the recognition of their labors may have been deferred by circumstances—mainly lack of opportunity—till later in life, they were actively engaged in their life work by the twenty-fifth year and had laid the foundation of success by the thirtieth. The late Dr. William Pepper, though one of the most earnest advocates of a liberal education for medical men as well as of thorough medical training, declared at the time of his ripest experience that any educational system was a mistake which would not allow the average man to enter upon actual practice at the age of twenty-three or twenty-four. It would prolong this paper unduly to quote his arguments, none of which however, was so convincing as his own life-history. Business men would, probably, assign a still earlier age. Among educators and scientists, there exists a considerable diversity of opinion; probably the majority would favor a lengthening of the period of preparation but it is questionable whether their personal biographies would support this opinion. On the whole, it would seem that the preparatory period should not occupy more than a third of the maximum duration of active life and that it should not extend much beyond the period of physiologic growth.

As a matter of abstract fairness, it may be argued that the advanced degrees are, at present, open to all college graduates on equal terms—let them accept or reject these terms as they please; if the A.M. or Ph.D. is not worth the sacrifice of a year or two of active life, why complain because one cannot eat his cake and have it too? But is this a wise attitude to assume? Granted that the privileges of the master or doctor and the esteem in which he is held by the community in no practical way exceed those enjoyed by the bachelor, long custom has established the post-graduate degrees, and they should stand for the best, ripest, most practical and wisest scholarship of the times. When the immature critic and student of other men's writings is eligible to a title that is denied to the man who creates literature that is deemed worthy of serious consideration, even though not of epoch-making value; when the laboratory worker who follows the lines laid down by others receives a tangible reward from which the pioneers of such study are often excluded; when field-work in science must radiate from a college rather than from a center which offers equal or greater scientific opportunities; when one museum or library yields not only information but a scholastic degree, while another, as good or better but not incorporated as part of a university, receives no such recognition; when second-hand knowledge of old-world linguistics and anthropology is placed on a higher level than original research, carried on independently, and dealing with similar problems

in American fields; when one must be a 'scholar' in the incorrect sense of the grammar school to obtain a scholastic recognition which can not be earned by the slower self-denial and effort of the man who devotes his leisure from the earnest of life to broadening his own intellect and extending the limits of human knowledge—may we not well ask if the reaction against honorary degrees has not carried the colleges too far in the opposite direction?

The necessary regulation of study *in præsentia* may be applied with only literal alterations, to study *in absentia*. There can be the same supervision by a faculty committee, the same minima of time required after the receipt of the bachelor's degree, the same inspection of work or formal examination, the same insistence upon a thesis the same or even higher standard of originality, the same precaution against too great concentration, even the same fees unless the college prefers to guard against the superficial appearance of interested motives. The post-graduate student *in præsentia* is seldom held to a definite schedule of attendance; the student *in absentia* needs only an extension of the same courtesy, and he may be required to report in person at stated intervals. Evidence of adequate resources for the special kind of study undertaken may be required and this could be supplied, so far as museums, libraries, art galleries, laboratories, mechanical workshops, etc., are concerned, by nearly very resident of a large city, while there is scarcely a region of the entire country which does not offer opportunities for one or more kinds of scientific field-work, in which original investigation is urgently needed. A slightly less formal requirement in regard to the customary 'two minor' subjects of study, would probably be wise in most instances and more careful inquiry into the probity and reputation of the candidate would be necessary than in the case of the resident student who is under the immediate and almost constant observation of the faculty, but it would seem that these various modifications of the regulations of study *in præsentia* are feasible, without too great effort on the part of the college authorities.

In conclusion, the writer would again emphasize, as the main plea of this article, that the present custom of limiting the post-graduate degrees to students *in præsentia*, places the intellectual consumer on a higher basis than the producer while it has a corresponding tendency to lower the scholastic value of the titles which ought, par excellence, to represent the highest attainments of the broadest scholarship.

MENTAL AND MORAL HEREDITY IN ROYALTY, VI.

By Dr. FREDERICK F. WOODS.

Bourbons in Spain.

Philip V. to the present day.

THE male or Hapsburg line having become extinct on the death of Charles II., the Bourbons came upon the Spanish throne. This group may be subdivided into six smaller groups:

1. Children of first marriage of Philip V.
2. Primogeniture line of Spain.
3. Children of Philip Duke of Parma.
4. Male line in the Two Sicilies.
5. The Carlists.
6. Children of Francesco de Paula.

1–2 I shall start with Philip V. and include in the group with him all his ancestors to the third or great-grandparent degree. This supplies 87½ per cent. of influence according to Galton's law. Next all the children of Philip V. will be included, as well as all their ancestors to the third degree. Then following down the line that corresponds to the throne, I shall treat of each fraternity in turn until the present Alfonso XIII. is reached. After this the other male lines (3–6) will be taken up. The daughters are also included, but not their children, as these are considered under the male lines in other countries—Austria, France, Portugal, etc. There are thirty-four persons in this group who require tracing. As each has fourteen ancestors in the third degree (two parents, four grandparents, eight great-grandparents) the total number of persons concerned is $(14 \times 34) + 34$, or 510. All are of value, even the remote edges, because any striking trait, insanity, genius or moral depravity exhibited in any ancestor should reappear further down; if not in some branch represented in its own country, then perhaps here in Spain. There are many of these second- and third-degree ancestors who have the worst possible epithets bestowed upon them, such as the type of Louis XV. of France, but strange as it may seem to those who discredit heredity, there are only two out of the five hundred and ten who have ever been called great or who could be ranked with the geniuses of a grade as high as 9.

These are Maria Theresa of Austria and her grandson, the celebrated Archduke Charles, who won so much distinction in his battles

against Napoleon. Maria Theresa comes in this group no nearer than a grandparent and then only twice, and as a great-grandparent only three times. In none of these does the genius reappear, though over on the part of the chart where most of her descendants are one sees higher marks for intellect. The Arch-duke Charles enters this group merely as a grandfather of the present Queen Regent of Spain, who is no unworthy descendant. The tracing of this higher mental strain, its origin and its reappearance, is to be found under Austria. So as regards genius the results are conclusive. The others are nearly all between 1 and 6, the great majority being below mediocrity, illustrating the intellect of the Bourbons which, as some one has said, never rose above cunning. Although this statement is not absolutely true, there seems to be a certain characteristic in the type of mind most often seen: low craftiness for intrigue, combined with laziness, debauchery, tyranny and often cowardice. This last is the slur we can least frequently bring against royalty. Whatever they are, they are nearly always brave.

The mental qualities are, for the most part, below the mean, while the moral qualities fall as far below the average as any of the worst regions in the older times; as bad as the Romanhofs in the seventeenth and eighteenth centuries. Charts dealing with this group show just how, if heredity be a great force, Spain was brought to her unfortunate fate, how nearly impossible it was that she should have escaped it.

Another important thing to notice is the strong variation in the moral qualities. It is very easy to separate the sheep from the goats. There are only a few about whom we should hesitate to say whether they were good or bad. I have attempted to so classify them in the following list:

*Philip V.
*Ferdinand VI.
†Louis.
Charles III.
Philip, Duke of Parma.
Marie Anne.
Charles IV.
†*Ferdinand I., Two Sicilies.
†*Philip, imbecile son of Charles III.
Maria Louisa, wife of Leopold II. of Austria.
†*Ferdinand VII.
Carlos, first pretender.
Isabella.
†Carlotta, Queen of Portugal.
Francisco de Paula.
†Isabella II. (Queen).
Maria Louisa Montpensier.
Alfonso XII.
Alfonso XIII.
†Ferdinand, D. of Parma.
†Maria Louisa.
*Elizabeth.
†Frances I., Two Sicilies.
Antonia.
†Ferdinand II. ('Bomba').
†Christina.
Carlotta, wife of Francis de Paula.
†Francis II., Two Sicilies.
Don Carlos VI.
*John.
†Don Carlos VII.
Alfonso.
†Elwora.
†Henrique.
Francis d'Assis.

There are thirty-five persons in this list, of whom fifteen were either cruel or dissolute or both. These have the mark † against them. There are at least seven either insane or showing the neurosis in a marked degree. These have the mark * applied to them. This leaves only thirteen free. Of these, four are known to have been indolent almost to point of disease. Thus, only about nine in the thirty-five were normal. This is a remarkably small ratio of normal and is less than found in any other country.

It will be shown that selection of the worst in each generation will account for it without other causes being necessarily introduced. We get some idea here of the extent to which a degeneration can be carried, and it is worthy of note that it may be perpetuated for a great number of generations, even when breeding in. There is no evidence that the in-breeding has led to sterility, as is usually contended by historians and students of the subject. Although the male line by way of the oldest sons ceased once at Charles II., and again at Ferdinand VII., nearly every marriage was prolific of many children, even among the closest blood relations, and one has but to glance at the 'Almanach de Gotha' for the current year to see the number of descendants that are being born to the closely inter-related families of Hapsburg, Bourbon and Orleans.

Summary of Modern Spain.

The occurrence just where they fall of every one of these modern Spanish Bourbons is compatible with the theory of mental and moral inheritance. There is no greatness springing up where we least expect it; there is no viciousness and imbecility that might not be explained from heredity alone. There is nothing that need be more than pure selection and repetition.

Of course we expect from Galton's law that, on the average, the descendants will show less of any peculiarity than the parents, and here we shall show that averaging all the descendants it is so, but all descendants would include other countries. Portugal, Austria, Italy, France, and including all these it will be shown in a later summary that there is a bettering of affairs from the time of Philip V. onward, but one must notice the artificial selection that took place in Spain. It was as if they were breeding mental monstrosities for a bench show. We see no diminution in either the debauchery or tyranny. The insanity does appear less at the bottom of the chart, but it will also be noticed that the early degenerates, Ferdinand VI. and Philip, son of Charles III., who were avowedly insane, had no children and the worst was consequently eliminated, while the worst moral depravity and laziness were not only perpetuated, but usually drawn from and in a double or triple way. This view of selection alone is important,

because this same family is usually considered to have run out through external circumstances and to have followed an easy road from opulence and luxury to indolence and decline.

I shall show that among all the races considered in this book a family never runs out except by selection, no matter what the condition of environment may be. It is far from my wish to assume that environment has done *nothing* in molding these characters, and especially the moral characters that fall under this group of modern Spain. If it has done *much* in order to account for a considerable number of excellent ones, and these often as good as any princes that have ever lived, we must assume that it, like the pedigree, was calculated to bring great variations. This probability will be discussed when all the greater groups are compared one with the other. If environment did have much to do with molding their individual destinies there is no apparent culminated inherited effect from it. After five or six generations the people are practically neither worse nor better than at first.

Nineteenth century estimates had no effect in lessening the cruelty and arrogance of Ferdinand II., 'Bomba.' He was as bad a tyrant as ever lived in the middle ages. His son was a man of the same type. The conditions in Portugal and Spain were not very different from those in Italy where Ferdinand lived, and yet Portugal and Spain show us nothing to be compared with the brutalities of this father and son. Ferdinand II. was no more a tyrant than his grandmother or some others among the Hapsburgs; Francis of Modena, for instance. Carlotta alone of those belonging to the immediate branch of the throne of Spain (occurring in the middle of the chart) would be rightly characterized by the word tyrant. Yet the conditions in Spain for the formation of an autocrat might be justly considered as conducive to this effect as were those of Italy. It will be noticed that the branches in Spain are practically free from this tyrannical type, except that Carlotta, daughter of Charles III., showed something of this character, and one of her sons, Migual, exhibited it in a high degree. She was one among four children to show the violent type. On the other or left side of the chart where the blood of the tyrannical Caroline of Austria is closest, we have Bomba and Carlotta, two of the same type in three children, and also Henrique, one in two, and Francis II., one in one. Imitation may have played a rôle, but then why did a certain definite number imitate and only a certain number do so?

What shall we say here of free-will? How could it have played any appreciable part in molding the characters of these scores of people, each apparently filling a little link in a chain, the destinies of which

seem as much the result of birth and breeding as the product of the most carefully conducted racing stable?

Evidence from Denmark.

The royal house of Oldenburg from which the kings of Denmark are descended covers, from Frederick II. to the daughter of Frederick IV., three centuries and ten generations. Including in each generation not only the reigning sovereign, but also his brothers and sisters, the number of names brought into this family is thirty-seven. In order to get the necessary material for heredity study, there have been added in each generation all the ancestors of every child back to the great-grandparents, so the number brought together in this group is raised by 132, or 169 represents the total.

With the exception of the first two kings, this period of Danish history covers what is known as the 'Age of Absolutism,' 1670–1848. A good idea of the sovereign rights at this time and the general characteristics of the rulers may be gathered from the following quotation:

> **Although the Royal Law conferred so absolute a power on the king, a power such as was perhaps not vested in any other sovereign in Europe, the autocrats of the Oldenburg dynasty—good-natured, upright and not more than ordinarily gifted as they were—exercised the prerogative, on the whole, with moderation and leniency, and the country had often reason to be thankful for the advantages secured to it during this period, especially when among the royal councillors were to be found men of talent and capacity.**

Good-natured, upright and not more than ordinarily gifted is a fair estimate for our thirty-seven members of the Oldenburg family taken as a whole. There are not more than three or four exceptions to this among them all. In other words, the Oldenburgs show no great mental and moral variations. Do the characteristics of the other 132, who, united with the male line, are the formers of the breed, warrant us in saying that this result is only what we might expect from the direct inheritance of the traits of these progenitors? It will be seen that the characteristics of these outsiders who represent the maternal side amply bear out such a belief.

In the pedigree of the Oldenburgs there is no Hapsburg, Bourbon or Romanhof insanity, or moral depravity. There is no Orange or Hohenzollern genius. In searching out the quality of the maternal blood as it was introduced all down the line, one finds no distinguished ancestry and few peculiar characters of any sort. Two of the queens had brilliant gifts of mind, one being also extremely unprincipled in her political actions. Aside from this there is little of interest in the ancestry. Frederick II., 1534–1588, was a headstrong and arbitrary ruler with too great a fondness for strong drink, but otherwise was not strange in any way and is not a striking figure in Danish

history. His consort, Sophia, however, was a woman much praised for her intellectual eminence.* From this union sprang Christian IV., the idol of Danish history and the only sovereign who ranks at all with the more able kings of other countries. There were six other children, but Christian is the only one who has left a distinguished record. Anna, the wife of Christian IV., descended from a comparatively obscure branch of the Brandenburg family, was a mild, sweet-tempered, charitable princess,† but not a conspicuous character in contemporary records. Their son, Frederick III., 1609–70, was a wise and shrewd sovereign, but of languid disposition. His temper was amiable, and his reign popular. The brilliant, haughty and vindictive Sophia Amelia was queen during this reign. It was she who imprisoned the king's half sister for twenty-two years, because, when trying on the crown, it is said, Eleonora Christian dropped it and injured a very fine jewel. The same authority gives us the anecdote that she ordered a noble executed, because he claimed she would fall in love with him. The Brunswick stock from which she came shows at this point no eminence of any kind; still we expect some of her six children to have been mentally gifted. The next generation gives us a rather mediocre showing, with Prince George, husband of Queen Anne of England, almost a fool. Ulrica Elenora, who married Charles XI. of Sweden and became the mother of the remarkable Charles XII., was the only one among the six children to represent the intellectual side of the family.

Christian V. 1646–1699, the eldest son, courageous, enterprising and chivalrous, was no ordinary man, but the strong tendency to ease and pleasure and the weakness he showed in being governed by others forbid us to give him a high rating for intellect when this is judged by the standard of outward achievements. His marriage brought in no mental uplifting, since the Queen Charlotte Amelia was from an 'obscure' region in the family of Hesse Cassel. Neither in the next generation (Frederick IV.) or the two following this (Christian VI. and Frederick V.) do we find any noteworthy mental variations. In all these generations a study of the chart will show the stock good but far from illustrious.

We now come to a very interesting anomaly in Christian VII., the only son of Frederick V. by his first wife Louisa, daughter of George II. of England. Among all modern royalty there is scarcely a feebler specimen of the human race than this poor little, half-mad, debauchee king. His type of mind was so purile and his self-restraint so weak that it seems only charity to consider him among the irresponsibles. From L. Wraxall and Walpole an idea may be obtained

* Allen, 'Hist. de Dannemark,' II., 29.

† L. Flamand, 'Danmarke Dronninger,' 1848, pp. 11, 12.

of his conduct during his visit to England, giving the positive impression that he was a degenerate of the worst type. He would be in just the place we might expect to find him, if he belonged among the older Romanhofs or modern Bourbons, yet there is none of this blood in him, nor is there any other equally bad. Christian VII. was a grandson of George II., and whether he got his bad qualities from him it is impossible to say. If he did he was certainly a great deal worse than George and much feebler intellectually. It is interesting in connection with heredity to note that Christian VII. was a first cousin of George III. who was insane, and also the first cousin, once removed, of the two imbecile sons of Augusta Princess of Brunswick, sister of George III.

Another more convincing bit of evidence in this connection is to be found in the neighboring House of Hesse Cassel; here we find another first cousin, once removed, of Christian VII., who became insane and died in early manhood. The observation that this man Christian, son of Charles of Hesse Cassel, is doubly descended from the suspected strain (Palatine House) makes it almost certain that we are dealing with an inherited insanity in all cases. Both the mother and father of this Christian of Hesse were grandchildren of George II., and consequently from the Palatine House. I almost forgot to mention Frederick William I., of Prussia, about whom Macaulay said, 'His eccentricities were such as had never been seen out of a mad house.' Frederick William was a first cousin of George II. and stands as near the actual Palatine insanity as a nephew.

These six cases would, if occurring in families of ordinary social grades, be sent to asylums and never make their way into the records as showing a congenital tendency. Since they stand apart from the other regions of neurosis such as the Spanish, Russian and modern Bavarian groups, at first we might suspect nothing, but here where we have the family tree and can look up the ancestry, curiously enough we find all related, and through the same source (Palatine) and this the only one of their many lines of descent in which there was insanity.

It should be noticed that the percentages for heredity among the insane run from 20 to 90, according to the observer, and this should make us think that the higher rather than the lower figures are more likely to be correct.

Besides this evidence we may mention the following facts: that the uncle of Christian VII., the Duke of Cumberland, was extremely cruel, and his other uncle, Frederick, Prince of Wales, was a dissolute specimen and William IV. of England was eccentric to say the least. Whatever we may say for hereditary influence, at any rate the bringing up of Christian VII. was pretty bad. He was in the hands of his step-mother, Juliana Maria of Brunswick, who is said to have used

every means to corrupt his morals and stunt his education that she might get the more power in her own hands. I only mention this to show a good example of the sort of cases that should make us bend strongly towards the importance of environment in molding the psychical form. It is the relative absence of such cases that has led to the view taken in this book. In spite of the fact that Christian VII. married his first cousin, related on the bad side of the house, since she was a sister of George III., his two children were not of the worst sort by any means, though in general we may say that one took after the father and one the mother. Louisa Augusta, the daughter, had relatively very little intellect, no ambition and a very quick temper, while Frederick VI., the next king, mild, affable and sensible, resembled his mother.

The remaining characters, Christian VIII. and Frederick VII., were merely examples of good normal men, liberal, popular and sufficiently able to fill their positions with honor to their country. There is nothing particularly interesting just here, so we can conclude the chapter of the Oldenburg dynasty with a glance back at the seventeenth century.

It will be noticed that there is one little region where the intellectual ratings are fairly high and that included in this group is Christian IV., the greatest of Danish kings. The only slight error from expected heredity is that the intellectual eminence fails to be perpetuated to quite the extent we might have expected in any of the children of Frederick III. Ulrica Elenora, the only gifted child, was 'distinguished for her knowledge.' She was the mother of Charles XII., of Sweden, and in him the genius was more than rejuvenated.

Aside from this heredity is very well satisfied in the study of this country, there being not more than one or two exceptions at most to what we might expect from the workings of this force. It is also important to note that the age of absolutism entirely failed to produce a type of cruel and arrogant kings. Had such a type been here engendered it would certainly have been ascribed largely to the environment in which they lived.

MENDEL'S LAW.

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NO discovery in recent years has aroused more interest amongst biologists than that of Mendel's Law. If subsequent investigations confirm it as those thus far made have done it can not be considered less than epoch-making. Its importance to plant breeders seems hardly less than that of the atomic theory to the science of chemistry. Professor Bateson, of Cambridge, says there is reason to believe that the plant breeder may eventually be able, by means of this law, to produce a desired hybrid type with the same degree of accuracy as the chemist now constructs a compound. It is impossible, on the threshold of such a discovery, to state just how far-reaching its importance is; we must wait for further investigation before hoping for too much.

As the history of this discovery is not yet generally known, it may be stated that Mendel's original paper was published in an obscure periodical at Brünn, Austria, in 1865.* This publication received slight notice until De Vries, in March, 1900,† published an exact counterpart of Mendel's theory, including some technical terms proposed by Mendel, and gave some of the results of his own researches to confirm the theory. Shortly after this, Correns of Germany and Bateson of England published results of their own work, showing that each of them had discovered the same law. Meanwhile the writer, working on hybrid wheats in this country, announced the law (but not the theory) in November, 1901.‡ The knowledge of this discovery has become general only during the past few months. It now remains for other investigators to apply it to their own results for confirmation or modification.

As the distinction between varieties and species can not always be drawn with exactness, and particularly since Mendel's law applies alike to crosses and hybrids, it seems justifiable, at least in a discussion such as the present one, to conform to the growing usage of biologists in this country by using the term hybrid to include crosses of all kinds, whether between varieties or distantly related species. I shall, therefore, use the term hybrid in this sense. In the following

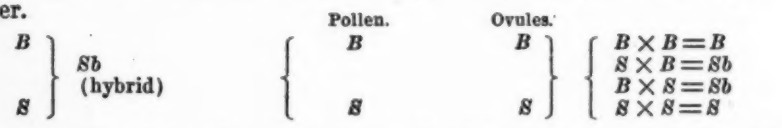
* *Verhandl. d. Naturf. Vereines*, Brünn, 1865.
† *Compte Rendus*, March 20, 1900.
‡ Bul. 115, Off. Exper. Sta., U. S. Dept. Agric.

discussion it is to be understood, unless otherwise stated, that the parent forms are all well-established varieties or species (*i. e.*, they come true to type from seed), and that the hybrids are close fertilized (either self-fertilized or fertilized by others like them).

Mendel's law is, briefly stated, as follows: In the second and later generations of a hybrid, every possible combination of the parent characters occurs, and each combination appears in a definite proportion of the individuals.

Mendel did not leave his work unfinished. He proposed a theory, or rather deduced one by a simple course of reasoning that renders the theory almost an established fact as far as results thus far secured are concerned, that explains the facts in the case in a brilliant manner.* As I have shown elsewhere, this theory explains most of the so-called exceptions to the law pointed out by several investigators. It explains so many apparent anomalies in such a simple manner that there is danger of applying it too extensively. This point will be brought up again below.

The theory may be illustrated as follows: Suppose the two parent varieties differ with respect to a single character. For instance, a variety of bearded wheat is crossed with one that is smooth (not bearded). When the hybrid thus produced forms its germ cells (pollen and ovules), the characters of the two parents separate, the beard-producing character passing into part of the pollen grains and part of the ovules, the smooth-head character of the other parent passing into the remaining germ cells. The character passing into any single germ cell being governed by chance, on the average half the pollen and half the ovules will receive the character of one parent, the other half that of the other. The results of this separation of characters may be illustrated by the following diagram, in which *B* stands for the beard-producing character and *S* for its opposite. The reason for the use of a small *b* in designating the hybrid will appear later.



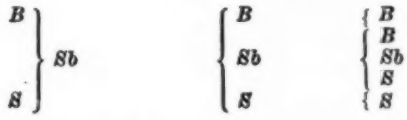
Since the ovules of each kind are offered both kinds of pollen, half of each, on the average, will be fertilized by one and half by the other kind of pollen, giving the four fertilizations shown at the right. (Abundant experience has shown that $S \times B = B \times S$.) From this it appears that one fourth of the progeny of the hybrid *Sb* will be like the parent *B*, one fourth like *S*, and one half like the hybrid itself.

* It seems that both Correns and De Vries had arrived at the same theory independently.

Nothing has been said so far concerning the external appearances of the hybrid, whether it is intermediate between the parents, or resembles one or the other of them. The efforts of investigators since the time of Kölreuter have been directed to the futile attempt to find some law which would enable the breeder to predict the appearance of this hybrid. In general, this can not be done, with our present knowledge. There are two cases to consider. In certain instances hybrids are strictly intermediate between the parents. In others they are unlike either parent. These cases will be noticed later. In the more common case the hybrid either shows a parent character fully developed or shows it not at all. A parent character which is fully developed in the hybrid is said to be 'dominant'; if it is apparently absent it is said to be 'recessive.' In my work with hybrid wheats, beards have always been recessive, hence the designation of the hybrid as *Sb*.

It will now be seen that, externally, the progeny of the hybrid will consist of only two types, one type (*B*), constituting one fourth of the progeny, being like one parent, and the other, constituting three fourths of the progeny, resembling the other parent. Of this three fourths, one part (*S*) is actually like one parent, and will produce progeny like itself. The other two parts (*Sb*) are hybrids, and will produce progeny of all the types, exactly as the original hybrid did. Plants of the type *S* may easily be separated from those of the type *Sb* by planting the seed of each plant separately, and noting the character of the progeny.

The above diagram may easily be extended to any desired number of generations. Extended to the third generation it is:



Assuming that each of the types *S*, *Sb* and *B* are equally productive, and mixing and sowing all the seed of each generation, the following table shows the percentage of each type in each generation to the sixth:

Generations.	*S*	*Sb*	*B*
1		100	
2	25	50	25
3	37.5	25	37.5
4	43.75	12.5	43.75
5	46.87	6.25	46.87
6	48.44	3.12	48.44

Here it is seen that the hybrid, based on a single pair of antagonistic characters, tends to split up into the two parent types. A hybrid

whose parents differ in one respect is called a monohybrid; a dihybrid is one whose parents differ in two respects; and so on. Dihybrids and polyhybrids do not tend to split up into the two parent forms, as will be seen later.

Heretofore, plant breeders have been producing hybrids, and then by selecting to type each year from the progeny, trying to fix new types. Let us see what light Mendel's law throws on this practice. In the illustration given above, if the breeder had selected the type *B* of the progeny of the hybrid, he would have had a fixed type at once. Had he selected for type *S*, he would have had a mixture of the pure type *S* with the mixed type *Sb*. (Professor Bateson proposes the useful terms homozygote for pure types like *S*, and heterozygote for mixed types like *Sb*.) Next year the homozygotes *S* would reproduce their kind only, while the heterozygotes would produce the three types *B*, *Sb* and *S* in the proportion 1:2:1. The second generation would therefore consist of *S*, 62.5 per cent.; *Sb*, 25 per cent.; and *B*, 12.5 per cent. This method of selection would never result in a fixed type unless the breeder should accidentally choose seed of the type *S* only. It has already been shown that the fixed type *S* could have been separated out at once by saving the seed of each selected plant separately, and observing which reproduced true to type. Nature fixes the type whether the breeder selects or not; heretofore, the breeder secured his fixed type by chance selection. With the knowledge of Mendel's law, he now selects his fixed type in a methodical manner, in the third generation.

Dihybrids are much more interesting, since they present the more usual case with which the breeder has to deal. With them, fixed types unlike either parent may be secured in the third generation. It frequently occurs that a breeder finds two characters in different varieties that he wishes to combine in a single variety. This is easily done when the characters obey Mendel's law. To illustrate this case I shall use characters which are of no particular importance, but for which I happen to possess experimental data. The principles are exactly the same for any characters that obey Mendel's law. Suppose we have a variety of wheat that has velvet chaff and another that has smooth heads (is not bearded) and that we wish to combine these two characters in a single variety. It is assumed that neither of the varieties has these two characters already; hence we have to deal with two pairs of opposite characters, namely, beards—no beards, and velvet—glabrous. We may, for brevity's sake, represent these characters by their initial letters, using small letters in cases where they are latent. In my work with wheats, beards have always been recessive, as stated above, and velvet chaff has always been dominant

(except in a single individual). Hence the cross $VB \times GS$ gives us the hybrid $VgSb$. When this hybrid produces pollen and ovules, the pair of characters V and G separate, V going to half the pollen grains and ovules, and G to the other half. S and B do the same thing, but without reference to V and G. Hence we have four kinds of pollen grains and four of ovules, as shown in the following diagram:

		Pollen.	Ovules.
VB	*VgSb*	*VB*	*VB*
		VS	*VS*
GS		*GB*	*GB*
		GS	*GS*

Here we have a mixture of four kinds of pollen offered to four kinds of ovules. On the average, one fourth of each kind of ovules will be fertilized by each kind of pollen, giving sixteen equally numerous fertilizations, as follows:

1. $VB \times VB = VB$.
2. $VS \times VB = VSb$.
3. $GB \times VB = VgB$.
4. $GS \times VB = VgSb$.
5. $VB \times VS = VSb$.
6. $VS \times VS = VS$.
7. $GB \times VS = VgSb$.
8. $GS \times VS = VgS$.
9. $VB \times GB = VgB$.
10. $VS \times GB = VgSb$.
11. $GB \times GB = GB$.
12. $GS \times GB = GSb$.
13. $VB \times GS = VgSb$.
14. $VS \times GS = VgS$.
15. $GB \times GS = GSb$.
16. $GS \times GS = GS$.

Here it will be noticed that (2) and (5) give the same result. Similarly, (3) and (9); (8) and (14); (12) and (15); and (4), (7), (10) and (13). We may, therefore, represent the hybrid and its progeny thus:

VB	*VgSb*	1 *VS*
		2 *VSb*
GS		1 *VB*
		2 *VgS*
		4 *VgSb*
		2 *VgB*
		1 *GS*
		Sb
		1 *GB*
		16 parts.

This diagram may easily be extended to later generations. *VS* will produce *VS*. The progeny of the type *VSb* will all have the character *V*, but one fourth of it will have the character *S*, two fourths *Sb*, and one fourth *B*; thus,

VSb	1 *VS*
	2 *VSb*
	1 *VB*
	4 parts.

In like manner the progeny of the other types may be written out.

Of the nine types produced from the hybrid, four of them, *VS*, *VB*, *GS*, *GB*, are pure, and will reproduce true to seed. They have no characters hidden in them to crop out in later generations. It will be noticed that each of these pure types constitute one sixteenth of the progeny of the hybrid. Four other types, *VSb*, *VgS*, *VgB*

and *GSb*, have each one latent character, and each constitutes two sixteenths of the whole. The type *VgSb*, having two latent characters, constitutes four sixteenths. In general, a type having n latent characters will be present in the second generation of any hybrid in a proportion 2^n times as great as any type having no latent characters.

Suppose now we sow all these nine kinds of seed and secure mature plants from each. Those of the type *GB* can easily be distinguished by their appearance. It can be selected out at once, as a new variety fixed in character. The case is different with *VS*, *VB* and *GS*. For example, if we attempt to select *GS*, we get also *GSb*, which has exactly the same external characters. But if we take all the plants with glabrous chaff and smooth heads (*GS* + *GSb*) and save the seed of each plant separately, we can separate the next generation by noting which plants reproduce true to type; for the seed of *GS* will produce *GS* plants only, while that of *GSb* will produce one fourth *GS*, two fourths *GSb*, and one fourth *GB*, according to Mendel's law. Or, since *GS* and *GSb* appear alike, one fourth of the progeny of *GSb* will be *GB* (glabrous and bearded), the remaining three fourths being glabrous and smooth. In the same way we can separate *VS* from *VSb*, *VgS* and *VgSb*, and *VB* from *VgB*.

Now *VS*, *VB*, *GS* and *GB* are all the possible pure (homozygote) combinations of the parent characters, two of them being identical with the two parents, the others constituting new varieties. The practical plant breeder, therefore, does not need to carry his hybrids beyond the third generation to secure all the possible results of a given cross, as far as new fixed varieties are concerned. It should be remembered that this is true only of characters that obey Mendel's law. It is plain, therefore, that it is a matter of the highest practical importance to ascertain how general this law is.

By the same methods outlined above, it is easy to ascertain what types would result from a trihybrid, and from hybrids of all higher orders. In the case of trihybrids, eight permanent combinations result, one like each parent and six new ones. Quadrihybrids give sixteen types, fourteen of which are new; and so on. In general, the number of new fixed types springing from a hybrid is $2^n - 2$, where n is the order of the hybrid.

The proportion of the various types in later generations of a hybrid is a matter of more than curious interest. We have already seen that, in the case of monohybrids, the later generations tend to split up into the two parent types. It was stated above that this is not so with hybrids of higher order. If we assume that each of the nine types (four homozygote and five heterozygote) resulting from a dihybrid is equally productive, the proportion of each of these types in each generation to the sixth is as follows:

PERCENTAGE OF EACH TYPE IN FIRST SIX GENERATIONS OF THE DIHYBRID *VB* × *GS*.

	First.	Second.	Third.	Fourth.	Fifth.	Sixth Generation.
VS, *VB*, *GS*, *GB*	each 0	6.25	14.06	19.10	21.97	23.46
VgS, *VgB*, *VSb*, *GSb*	each 0	12.50	9.38	5.47	2.93	1.15
VgSb	100	25.00	6.25	1.56	.39	.10

Hence, if we sow all the seed of each generation, it is seen that each of the homozygote types approaches 25 per cent. of the whole, while all the heterozygote types approach zero, and the larger the number of latent characters in a type, the more rapidly it decreases in proportion. In trihybrids, we should have eight homozygote types, each increasing toward 12½ per cent. of the whole. Hence the generalization, a hybrid of the *n*th order tends to split up into 2^n fixed types, all types not fixed tending to disappear. The effect of such a law, in the case of accidental hybrids between species and varieties in the wild state, can not fail to be important in the evolution of species. I leave the discussion of this interesting phase of the subject till the law is more generally confirmed. In this connection it may be well to state that, at the recent international conference of plant breeders in New York, Professor Bateson asserted that Mendel's law has been found to hold in every case where it had been thoroughly tested.* The groups in which these tests have been made are so varied, representing both plants and animals, that the presumption in favor of the generality of the law is strong enough to warrant breeders in searching for it everywhere.

It has been urged by certain breeders that, even if the law is general, it can not be put to practical use by breeders; for it nearly always happens that the varieties crossed differ in an indefinite number of respects, and we should therefore get so vast a number of resulting types that no two individuals could be classed together. This objection is not altogether valid. In the first place, if we take any established variety and examine the individuals closely, we find no two of them alike. Hence, even if the variety we are trying to produce must consist of an indefinite number of types differing only in minor details, we are no more than duplicating the actual conditions existing amongst present useful varieties. In the second place, a very common problem of the breeder is to unite two characters found

* This does not agree with Correns (*l. c.*). I think, however, that the cases to which Correns refers may be explained by means of Mendel's law.

in related varieties, where the remaining characters of both varieties are unimportant. Hence, in practice, we have in reality to deal with dihybrids in many cases. It should also be remembered that, if we treat a hybrid as a dihybrid, neglecting all but the two characters of most concern, the type we select actually splits up into fixed types with reference to all other characters, so that in a few generations we can secure uniformity, even in minor characters, by selection.

There is a very interesting phase of the subject which, for the sake of clearness, has been purposely overlooked in what has been said above. We have dealt only with the case in which a parent character appears in the hybrid in a fully developed state, or is not apparent at all. This is actually the case with the characters discussed above. Cases are known, however, in which both of a pair of opposite characters appear in the hybrid. This may result in a form intermediate between the parents, as I found to be the case when I crossed short-headed club wheats with the ordinary long-headed varieties. The same phenomenon appeared in crosses between varieties with red chaff and those with white chaff. Sometimes, in crosses between white and red flowers, for instance, the heterozygote types are variegated. It is easy to see that this fact may have an important bearing on the flavor and other characters of hybrid fruits, such as apples, peaches, strawberries, etc. It is highly probable that a great majority of these fruits are heterozygote in character, which fact would explain their well-known variability when grown from seed. It would naturally be expected, since flavors are due to the presence, in various proportions, of certain chemical substances, that entirely new flavors should be found in seedlings of this character, for in almost every seedling we should have a new combination of the flavor-giving substances.

One reason why Mendel's law was not discovered long ago is doubtless to be found in the fact that the large majority of seedlings that have come under the breeder's eye have had heterozygote parents of unknown constitution. If all our leading commercial varieties had been commonly close-fertilized, the law would long ago have forced itself upon us. Professor Bailey's remarkable and careful work on hybrid squashes and pumpkins probably came to naught for this very reason. Had he done the same work with varieties that are normally close-fertilized, he would probably have discovered this law. He was on the right road, but he was in the wrong vehicle.

Let us consider what results would follow the growing of apple seed generation after generation with close-fertilization, if the characters of the apple obey Mendel's law. We start with a tree that is already multihybrid. Suppose it to consist of N pairs of opposite character, A–A', B–B', $\cdots$, N–N'. The hybrid and the first genera-

tion of its progeny would then be, supposing the primed characters to be recessive:

$$Aa'\ Bb'\ Cc'\ldots\ldots Nn' \text{ (hybrid.)} \left\{ \begin{array}{l} ABC\ldots\ldots N,\ ABC\ldots\ldots Nn',\ ABC\ldots\ldots N', \text{ etc.} \\ \text{(progeny.)} \end{array} \right.$$

The total number of types occurring in the progeny is 3^n; the number of fixed types (homozygote) is 2^n. The number of types with r latent characters is $nCr2^{n-r}$, where nCr is the number of combinations of n things taken r at a time. Only one type, namely, $A'B'C'\cdots N'$, consisting entirely of recessive characters, could be selected out without getting with it one or more heterozygote types. But by saving the seed of each tree of this generation separately, and observing which, with close fertilization, would reproduce true to type, we could at once secure, in fixed from, all the 2 homozygote types.

If the tree happened to be of the type *VgSb*, discussed above, in which all the characters of its original parents are present, the above process of analysis would give, not only all its original parents, but all possible combinations of them, and each in a form that would reproduce true to seed, if self-fertilized. If it were of the type *VgB*, in which some of the parent characters are missing, it would give all the original parents whose characters are all present, together with all their combinations with characters that are present from other parents, some of whose characters are missing. Since apples are confessedly many times multihybrids, it is probable that a very large number of seed would have to be used to secure all the types capable of resulting from combinations of the N pairs of characters.

Suppose we neglect all but essential characters; we might, in the case of the apple, reduce the number of types to a point which would make the task a possible one. If this were done, it would mean much to the plant breeder. Having a large number of fixed varieties of apples, supposing, of course, that Mendel's law holds, we could select parents with a view to producing any combination of characters we desire. Did not Downing, many years ago, assert that much better results could be secured in producing new seedling apples by using seed from strains that had already been propagated several generations from seed? And why? Possibly because the continuous propagation from seed tends to produce pure strains. If the seed used were always produced by self-pollination, there certainly would be a tendency to pure strains if Mendel's law applies. This problem is worth working out, both from practical and from theoretical grounds. It could be done more easily with strawberries, or with some of the common ornamentals that do not reproduce true to seed. This method of analysis is one way of testing Mendel's law in such groups.

This subject is too new to permit of any useful generalizations

respecting animal breeding. True, Mendel's law has been found to apply with animals so far as the test has been applied, but it will be some time before much use can be made of it in that direction. It will take years to overcome the prevailing prejudices of animal breeders in favor of old-time theories that govern practice in that line.

Exceptions to the Law.

Millardet and others have given accounts of hybrids that immediately split up into the two parent forms, having no, or very few, hybrid progeny, and these have been cited as exceptions to Mendel's law. I have elsewhere* shown that Mendel's theory of the separation of parent characters offers a perfectly rational explanation of these cases. If a hybrid is obtained from two varieties each of which prefers its own pollen to that of the other, the resulting hybrid must split up at once into the two parent types, if it obeys the law in question, since each of the two kinds of ovules produced by the hybrid, being offered both kinds of pollen, is fertilized only by its own kind. Yet even in these cases, according to the laws of probability, it ought occasionally to happen that such a hybrid would produce a few hybrids, for it would occasionally happen that an ovule would be offered only one kind of pollen. And this is what actually occurred in hybrids of this class reported by De Vries. A few hybrids occurred along with an excess of both the parent types.

Likewise, when two varieties are crossed, each of which prefers the other's pollen, there will also be an apparent departure from the law; for in this case each of the two kinds of ovules on the hybrid will be fertilized only with the opposite kind of pollen, giving all heterozygotes. Such a hybrid will appear to be fixed in type at once. Such cases have been reported by many observers, including Darwin. In this case it may occasionally happen in later generations of the apparently stable hybrid, that an ovule will be offered only its own kind of pollen and we then get a reversion to one of the parent forms. We may yet find that many sports are to be explained in this manner.

Breeders frequently report entirely new characters in hybrids. If these actually occur we must look further than Mendel's theory for their cause. It must not be overlooked, however, that if the two parents of a hybrid are themselves heterozygote hybrids, Mendel's theory would call for characters unlike any of the visible characters of either parent. In this case, all latent characters in both parents would necessarily crop out in the second generation of the hybrids. We can not dismiss Mendel's theory in such cases until it has been demonstrated that the parents have no latent characters in them.

* *Science*, October 31, 1902.

It is very probable that many supposed new characters are merely the peculiar result of the blending of opposite characters, neither of which 'dominates' the other; or it may be that the blending of two unrelated dominant characters gives rise to new characters. As an instance of how this might occur, the presence of two chemical substances, one derived from each parent, might give new flavors in fruit, or new colors in flowers. Bateson has demonstrated a case of the latter and shown that the new and radically different color is not a new character at all, but a blending of parent characters which obey Mendel's law perfectly.

Is it not possible that Mendel has also shown to us an explanation of bud sports? These sports are notoriously common on plants known to be hybrids. May not the separation of parent characters occasionally occur at stages of growth other than the formation of germ cells? If such is the case, bud sports at once come under the law.

It does not seem improbable that, once in a while, the parent characters might fail to separate in the usual manner on the formation of germ cells, so that we might have a few cells inheriting both of a pair of opposite characters, and this might extend over a series of generations before the separation finally occurred. Under such conditions, a recessive character might be carried over any number of generations without showing itself, finally cropping out and giving a case of atavism. Perhaps this is the explanation of atavism. It would be interesting in this connection to know if atavic characters are ordinarily recessive.

It is clear that we have before us a working hypothesis that offers a possible explanation of a large number of phenomena heretofore absolutely inexplicable. It will require time to test the hypothesis, even in the limited number of cases suggested above.

The only data thus far published in this country that may be used as a direct test of Mendel's theory are those I secured from hybrid wheats.* At the time these data were arranged for publication similar work in Europe was unknown in this country; they were merely arranged to show that similarly bred hybrids split up into the same types, each type tending to occur in a definite proportion. In all my hybrids characters were present that apparently separated in a manner different from that called for by Mendel's theory. Fortunately, however, the data referred to may easily be arranged to test the applicability of this theory to two characters, namely, beards and velvet chaff. In five out of fourteen crosses one parent was bearded and the other smooth, two of the five being reciprocals. Beards being recessive, theory would call for 25 per cent. of bearded plants

* Bul. 115, Off. Ex. Sta. U. S. Dept. Agr.

in the progeny of the hybrids. The seed of each hybrid plant was kept separate and sown as a plat. In three plats from a cross between Valley (♂), a bearded variety, and Little Club (♀), there was an average of 25.7 per cent. of bearded plants. Eleven plats of the reciprocal cross averaged 25.2 per cent. beards. Six plats of a cross between Little Club and Emporium gave bearded plants to the extent of 24.6 per cent.; three plats of Lehigh × Red Chaff, 25.9 per cent.; and seven plats of Turkey × Little Club, 30.8 per cent. In the last example there were two aberrant cases, the remaining five lying between 25 and 29 per cent.

Much evidence of a similar nature has been brought forward by De Vries, Correns, Bateson and others, in addition to that given by Mendel. These investigators worked with widely different groups of plants and animals.

Thus far, no one has shown definitely that Mendel's theory is inapplicable to a single case. Correns, however, mentions hybrids which do not behave exactly as called for by theory, but I am not sufficiently familiar with the details concerning them to discuss them here. In my own work I found that the color of the chaff and the length of the head behave in a manner most easily explained by a modification of Mendel's theory. Instead of the pair of opposite characters, long heads and short heads, separating completely on the formation of pollen and ovules, they seemed to separate in all possible proportions, giving in the next generation a series of plants having heads of every possible gradation of length between those of the two parents, and even extending in both directions beyond the parents. In my work I arbitrarily separated the hybrids into three groups—long, semi-long and short heads. As this separation was entirely arbitrary, the results are very irregular, and the original figures do not represent very accurately the actual facts with reference to this character. Exactly the same thing occurred with reference to color of chaff. I have not yet had the opportunity of examining the third generation of these hybrids, so that it can not yet be stated definitely that they really form an exception to Mendel's law.

As is the case with any startling discovery, we are apt either to minimize its importance or to extend its application much beyond legitimate bounds. I fear I shall be accused of the latter. But this new thory is so suggestive and offers a rational explanation to so many hitherto enigmatical phenomena, that a few suggestions as to its possible application can do no harm.

THE PROGRESS OF SCIENCE.

THE CONVOCATION OF SCIENTIFIC SOCIETIES

THE American Association for the Advancement of Science and in affiliation with it the American Society of Naturalists and more than twenty special scientific societies will meet in Washington at the end of December and beginning of January. This first of the convocation week meetings of scientific societies will probably always be an important date in the history of science in America. It is a truism to say that the progress of science consists in cooperation among men of science. For such cooperation two main agencies exist which are equally essential—the printing press and personal contact. Books and journals bring the whole world together and make science truly international; but the coming together of the men of science of the country is necessary for a national spirit. This spirit has suffered in the past owing to the dispersion of our scientific workers over an immense area with no one center, such as exists in all foreign countries. A year and a half ago the American Association and its affiliated societies met for the first time west of the banks of the Mississippi, and a year ago the American Society of Naturalists and its affiliated societies met for the first time west of the Atlantic seaboard. Now these two associations and the twenty special societies affiliated with them will for the first time meet together, and we are about to have our first national congress of scientific men.

A convocation week in mid-winter for the meetings of societies has been provided by the action of the leading universities and other institutions, which have extended their Christmas holidays or made other provision by which the week in which the first of January falls is left free from academic exercises. Under these circumstances it is a duty as well as a privilege for all to attend the meetings who are able to do so, and there is no doubt but that the number of scientific men at Washington will be the largest that has ever been gathered together in this country. While the special societies are for scientific experts, it should be remembered that the American Association is concerned with the diffusion as well as with the advancement of science. Its membership is divided into fellows and members. The former class consists of those who are engaged in research, while the latter class contains those who are interested in science. Readers of this journal can obtain information in regard to membership from the permanent secretary, Dr. L. O. Howard, Cosmos Club, Washington, D. C. It may be said here that the dues are only $3 a year, and that members receive free of charge the weekly journal, *Science*. We should be pleased if ten thousand readers of this journal would join the association. They would receive a full return for the small membership fee and would at the same time perform an important service in keeping men of science in contact with the large public from which sympathy, support and recruits must be drawn. Science in the United States has suffered seriously from the fact that there is too great a gulf between the professional man of science and the educated public. In Great Britain there exists a class bridging this gulf, and from it have come men such as Darwin, Rayleigh, Avebury, Huggins and many more. Much would be accomplished for the promotion of such a class here if the member-

ship of the American Association were made twice or ten times what it now is.

Both the scientific man and those only interested in science will be amply repaid by attendance at the Washington meetings. Indeed no scientific man can afford to be absent. Those who wish merely to keep in touch with the forward movement of science will profit much from attendance. They can visit Washington at a favorable time at greatly reduced rates and hotel charges, and there will be much to interest them in the programs. President Roosevelt is honorary president of the local committee and it is expected that he will open the meetings. It would be impossible to quote the titles of the hundreds of papers that will be presented, but their general character is indicated by the names of some of those who will give official addresses: Before the Association President Asaph Hall, and before the sections of the association vice-presidents Hough, Weber, Derby, Culin, Welch, Franklin, Flather, Nutting, Campbell, and Wright; before the Astronomical and Astrophysical Society of America, President Simon Newcomb; before the Chemical Society, President Ira Remsen; before the American Society of Naturalists, President J. McKeen Cattell; before the Botanical Society, President J. C. Arthur; before the Geological Society, President N. H. Winchell; before the Psychological Association, President E. C. Sanford, etc. Public lectures will be given before the Association by Professors Russell and Heilprin on the volcanoes of the West Indies, and before the Naturalists by Dr. Merriam on protective and directive coloration. The discussion before the Naturalists is on 'How can endowments be used most effectively for research?', the speakers including Professors Chamberlin, Welch, Boas, Wheeler, Coulter and Münsterberg. These are only a few of the hundreds of scientific men who will be present and present papers or take part in the discussions of the meetings, which promise to be more interesting and important than any ever before held on this continent.

THE CARNEGIE INSTITUTION.

It might be expected that after the annual meeting of the trustees of the Carnegie Institution on November 25, some statement could be made here in regard to the policy of the institution. Nothing has, however, been made public beyond the news item given to the reporters to the effect that $200,000 had been appropriated for the work to be determined by the executive committee, $40,000 for publication, $50,000 for administration and $100,000 for a reserve fund. The institution will, however, publish a year-book, which will doubtless contain various matters that have hitherto been kept secret, such as the names of members of the advisory committees of scientific men and their reports. Though no official announcement has been made, it appears that certain grants have been approved by the executive committee. Thus the medical papers report that $10,000 a year has been appropriated to revive the 'Index Medicus,' formerly compiled under the direction of Dr. John S. Billings, now vice-president of the institution.

A form of application for grants has been printed and approved by the trustees, to which it seems that men of science are likely to object, if indeed reputable men of science will consent to sign it at all. This form requires scientific men to promise to begin the research 'forthwith and to prosecute it diligently,' not to publish their results elsewhere if the institution wants them and to give all their apparatus, material, collections, etc. to the institution. These and other conditions on the contract seem to be almost an affront to men of science, calculated to profit the Carnegie Institution at the expense of others. It

is perhaps premature to criticize the institution when so little is known in regard to its plans. So long, however, as these are not disclosed the institution must be judged by what has been made public. It is known that when the Marine Biological Laboratory at Woods Hole asked for the assistance that it so well deserves, the executive committee replied that they would assist the laboratory if it were given to them, and the corporation actually voted to give the laboratory to the institution.

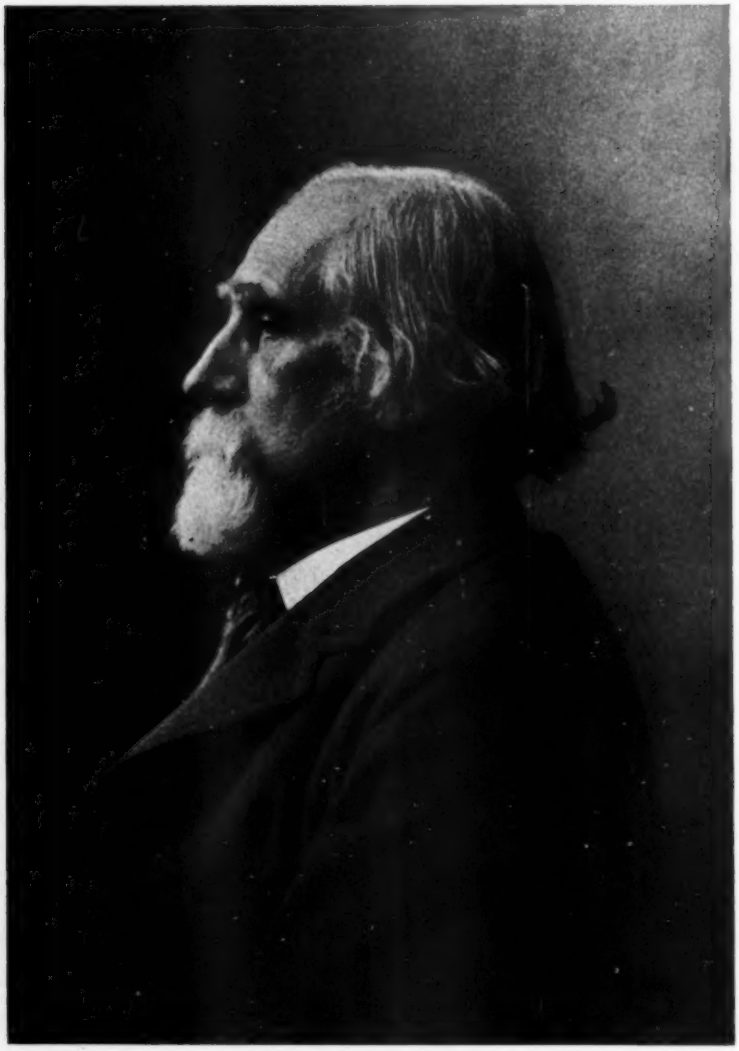
We hope and believe that the appearance of seeking to aggrandize the Carnegie Institution at the cost of other agencies and of men of science, instead of cooperating with them for the advancement of science, is not real, but due only to lack of information in regard to the purposes of the institution. It is, however, but just to men of science that this information be made public at an early day. The lines of Mr. Carnegie's great gift were drawn broadly and generously, and in spite of the apparent mistakes that have been made, there is every reason to believe that the institution will be conducted in the spirit in which it was founded.

THE BRITISH EDUCATION BILL.

IT is a striking fact that the British parliament and the British people have in recent months been occupied chiefly with the concerns of primary education; and it is at the same time somewhat depressing to consider that almost no attention is paid to the subject here, the daily papers publishing fuller details of the last divorce case connected with the nobility than of the bill now before parliament. The bill which the House of Commons has sent to the House of Lords is really of very great interest. The affairs of primary education in Great Britain and Ireland have been somewhat anomalous. About half the children who receive free education attend the board schools, supported by the state and by local taxation, and corresponding pretty closely to our public schools. The voluntary schools, controlled chiefly by the church of England, provide for the other half of the children. They do not share in the local taxes, but when they remit tuition fees they receive from the general government a *per capita* grant of $1.25 for each student. The main feature in the bill passed by the House of Commons is the support of the voluntary schools by taxation, local and central, leaving them largely under the control of the church. This is undoubtedly a step in the direction of local and state control, and might be supposed to be acceptable to the liberal and radical parties and distasteful to the conservative and church parties. The exact reverse is the case; the bill has been made the chief measure of the government, and has been bitterly opposed in and out of parliament by liberals and nonconformists. It is claimed that it is a subversion of the principles of free government to tax the community for schools which are conducted by the church and which teach the creed of the church. As a matter of fact the denominational schools are already supported in part by taxation as is also the established church. It, however, appears to be a real hardship that the children of dissenters must be sent to schools where rites are practised that are distasteful to their parents. Our public schools are so completely exempt from denominational control that we can scarcely understand the position of the bishop of London when he says in a public address 'an undenominational education is a rotten system.' The bill will doubtless be passed, and the church will for a while receive from rates what has hitherto been paid by subscription. But it seems almost evident that the voluntary schools will be less subservient to the church than hitherto and that a long step has been taken in the direction of popular control.

OGDEN N. ROOD.

IN the death of Professor Ogden N. Rood, America has lost one of its few men of genius. He belonged to a type of the scientific man which seems to be disappearing. Scientific work is becoming in its methods somewhat like any other business and the man of science, the man of letters and the artist no longer belong to distinct types. They take their places in an organized army with generals, petty officers and privates; there is no longer place for the adventurer. But Rood was a man of genius, in the sense in which the word is popularly and perhaps properly used. There was something unexpected and unaccountable



OGDEN N. ROOD.

both in his scientific discoveries and in his personal traits. He was not a mathematician, he was not always familiar with work that had been done in the same direction as his own, he did not have assistants nor use the ordinary machinery of research. But he had ideas, which he worked out with originality and persistence, devising his own methods and making his own instruments.

Rood was born in Connecticut on February 3, 1831, his father being a clergyman. He was dismissed from Yale College for some student escapade —it is said because he stopped chapel by shooting an arrow into the face of the college clock—and it was with some satisfaction that he received the doctorate of laws from Yale University on the occasion of its bicentennial. He graduated at Princeton in 1852, and spent several years in study abroad, a course not common in the fifties. For five years he was professor in a small denominational institution at Troy. Then at the early age of thirty-three he was called to Columbia College and at

New York, April 27th, 1901.

My dear Professor Cattell:

Yesterday I read with great delight the address of Professor Woodward on "Observation and Experiment," that recently appeared in "Science." The literary execution of this paper is so good, it is so bright and entertaining, aside from its thought, that it seems to me that other readers than those of "Science" would be glad to see it. Could it be copied in "Popular Science"?

Always yours,

O. N. Rood.

the same time was made a member of the National Academy of Sciences. For thirty-eight years he held the professorship of physics, while Columbia developed from a small college into a great university, his own work adding much to its fame. His researches in experimental physics are too numerous even for naming, extending as they do over a large part of the science. They include work on photography, projectiles, vacuum pumps, electricity and especially physiological optics. Every one of his papers, perhaps seventy-five in number, embodied a new idea, worked out with ingenuity and persistence. He was also an artist, his water-color sketches being highly esteemed, and was perhaps especially interested in those phases of research that required the knowledge of the physicist, the psychologist and the painter.

Rood was one of the marked men of Columbia University and of New York City. Striking in appearance and in manners, possessing and possibly affecting certain peculiarities, working behind locked doors, sometimes living with his family and sometimes not, in part a recluse, though not averse to congenial company or an evening at the Century Club, he possessed altogether a notable personality. He was a good enemy and a good friend; and the present writer regards it as a special privilege that he was counted a friend.

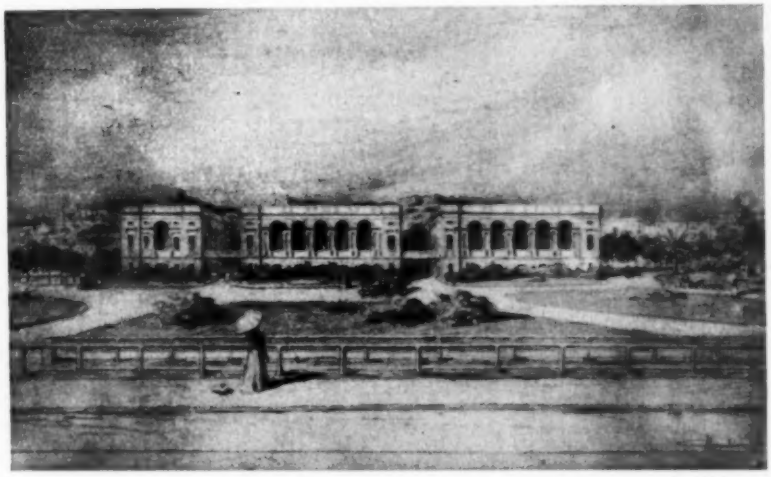
THE PROPOSED ENLARGEMENT OF THE NAPLES STATION.

THE untiring energy of the founder of the Naples Station, Professor Anton Dohrn, has made it possible, with the help of generous friends, to add a new building to the two already existing ones. When first started, in 1873, the station consisted of a single building—the middle one of the three in the accompanying figure. It soon became necessary to add another part, and the building to the left in the figure was tend erected. The station has now outgrown both of these, and another building is about to be added. As shown in the figure the new part will be a duplicate of the oldest building as far as the exterior is concerned. In the interior, however, the arrangement will be entirely different. It is proposed to have a large laboratory on the first floor devoted to physiological research; another on the floor above to physiological chemistry. In addition there will be a large number of private rooms for zoologists and physiologists. A new feature will be rooms in which the

THE NAPLES STATION.

water in the aquaria can be kept at any desired temperature throughout the year. This will give an opportunity, not only for keeping alive a number of different kinds of animals that will not live at ordinary temperatures, but will also give to the investigator a chance to carry out important researches on the effect of different temperatures on marine forms.

The addition to the station will double its working capacity, since the new part will be entirely devoted to investigation, while in the two older buildings there are the public aquaria, the collecting department, and the library. Zoology, botany physiology,



THE PROPOSED ENLARGEMENT OF THE NAPLES STATION.

physiological-chemistry and psychology will benefit the world over by this enlargement of the Naples Station. Professor Dohrn deserves to be heartily congratulated that his labors have been crowned by success. May the time come before long when the station will be made symmetrical by the addition of a fourth building!

ARE SOLUTIONS MECHANICAL MIXTURES OR NEW SUBSTANCES?

IN a recent volume Duhem sketches the development of our ideas in regard to solutions. If we drop a lump of sugar into a glass of water, the sugar disappears and we have in the tumbler a colorless liquid which looks like water, but which has a different taste. We are all agreed as to the fact, but there has always been a difference of opinion as to what became of the sugar. One view is that the sugar and water are still there, but so finely divided that we do not see the sugar. If we grind a little dry sugar together with a good deal of charcoal, we get a black mixture in which the eye does not detect the sugar though the sugar is unqestionably there. Another view is that we have neither water nor sugar in the tumbler, but a new substance having properties differing more or less completely from those of the sugar and water. This is the view that we take in regard to sugar itself when we speak of it as made up of charcoal and water. The first view, of a mechanical mixture, was held by the Greek atomistic philosophers under Epicurus while the second view was defended by Aristotle and the peripatetic philosophers. Through the Middle Ages, the views of Aristotle prevailed; but Bacon and Descartes brought the atomistic view to the front

again, while Newton modified the views of Descartes by substituting assumptions in regard to mutual attractions and repulsions for assumptions as to the shape of the atoms. In the second half of the nineteenth century, the application of thermodynamics to chemistry has led to the discovery of new laws, and these discoveries have been made without assuming anything in regard to atoms. The natural tendency is therefore to reject the atomic theory as a superfluous hypothesis. The only distinction that we can draw between chemical compounds, like sugar or salt, and solutions, such as a mixture of sugar and water, is that the composition of the solutions can vary continuously while the composition of the compounds can not. The natural inference is that solutions are to be looked upon as compounds or new substances with varying composition. The scientific world has thus come back to the view of Aristotle. The matter stands now as it stood centuries ago. One school still holds the views of Epicurus, another stands ready to break a cudgel for Aristotle. Even now we do not know what happens when we put sugar in our coffee though we know why we do so—except where it is merely a matter of habit.

SCIENTIFIC ITEMS.

We regret to record the death of Professor Henry Mitchell, the eminent engineer, and of Major Walter Reed, well known for his researches on the relation of the mosquito to yellow fever.

Dr. W J McGee, ethnologist in charge, Bureau of American Ethnology, has been appointed to represent the United States on the American International Archeological Commission.—Professor J. Willard Gibbs, of Yale University, has been elected a corresponding member of the Munich Academy of Science.

It is reported that the Nobel prizes for this year will be awarded as follows: In chemistry, to Professor Emil Fischer, of Berlin; in physics, to Professor S. A. Arrhenius, of Stockholm; in medicine, to Professor Niels E. Finsen, of Copenhagen, and to Major Ronald Ross, of Liverpool. The value of these prizes, it will be remembered, is about $40,000 each.